

# Psychological Bulletin

## A METHODOLOGICAL REVIEW OF EXTRA-SENSORY PERCEPTION<sup>1, 2</sup>

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### I. INTRODUCTION

In a recent issue of the *Journal of Parapsychology* (29) the editors make the following statement concerning the need for a comprehensive review of experimental work on extra-sensory perception:<sup>3</sup>

"One significant feature stands out in the four years of criticism to which the ESP research has been subjected: Up to the present no critic has attempted a thorough and a comprehensive evaluation of the research as a whole . . . The rapid expansion of the explorations in this field makes it particularly urgent that the primary and fundamental question of the occurrence of extra-sensory perception be established once and for all, or the essential weakness of the claim for it be pointed out by a very much more drastic analysis than has yet been forthcoming" (p. 79).

The present review has been designed to meet this need from the standpoint of experimental methodology. The reviewer has attempted to evaluate what he considers to be the important methods, both historical and modern, and to criticize experimental methods with the purpose of improving them. Since the problems of telepathy and clairvoyance have been raised again, he believes that it is rather important that they should be settled, in so far as they are capable of solution, by the application of acceptable scientific methods.

<sup>1</sup> This paper has been read and approved by the Stanford Committee on Psychical Research.

<sup>2</sup> The assistance of the late Professor John Edgar Coover in compiling this review, given through personal conversations and especially through his complete card index of titles on the history of telepathy and clairvoyance research, is gratefully acknowledged.

<sup>3</sup> Defined in the glossary of common terms found at the end of each number of the *Journal of Parapsychology* as: "Response to an external event (perception) not presented to any known sense." Hereafter referred to as ESP.

## II. HISTORICAL SUMMARY OF EXPERIMENTAL METHODS IN THE STUDY OF TELEPATHY AND CLAIRVOYANCE

### A. ERRORS IN EXPERIMENTAL METHODS

Much of the material to be reviewed under this heading may be found in greater detail in Coover's extensive monograph (22), which covers the period from 1882 (establishment of the British Society for Psychical Research) to 1917. Prince (81), Bird (9, 10), Murphy (73, 74), and especially Rhine (83) have also reviewed experiments on telepathy and clairvoyance during this period and up to the modern work on ESP, which began at Duke University in 1930. Without attempting to evaluate these early experiments with respect to the ESP hypothesis, it is possible to reconsider them from the aspect of their contributions, at the time, to the knowledge of unsatisfactory experimental methods in parapsychological research. For example, the early work of the British Society for Psychical Research served to emphasize the factor of involuntary and unnoticed sensory cues as the basis for some positive results in telepathy experiments. These involuntary cues were utilized in kinesthetic, tactual, auditory, and visual modalities. The work of the British and American Societies also brought about a study of "mental habits," preferences, and "coincidences" as productive of questionable positive results. Finally, some of the recent work of the British Society, although it has not received the critical attention due it, has illustrated the necessity for making independent records of guesses and material-to-be-guessed in telepathy and clairvoyance experiments. Since Richet's (91) introduction of methods demanding large numbers of trials in telepathy and clairvoyance experiments, this point has become of some importance.

#### 1. *Minimal and Subliminal Sensory Cues.*

(a) *Kinesthetic and Tactual Cues.* According to Jastrow's recent paper (51) on Chevreul's contributions to the history of suggestibility, involuntary muscular movements have been and continue to be associated with supernatural causes. Barrett and Besterman (4) made an exhaustive study of "dowsing" or use of the divining rod and reached the conclusion that the movements of the rod were caused by the dowser's own unconscious or involuntary muscular movements. No good evidence for other than normal causes for the movements was obtained. Faraday (31) and Chevreul (21) are responsible for the definitive studies on invol-

untary movements as related to "psychic" manifestations. Faraday showed that "table tipping" and "rapping" were caused by unnoticed pressures applied to the table by the sitters; Chevreul, that the swings of a pendulum held in a suggestible person's hand were caused by involuntary movements. Suggestions, even though the subject was not consciously aware of receiving them, determined the direction and amplitude of the swings.

Involuntary movements also served as uncontrolled cues in telepathy experiments. In the middle of the last century, the so-called "willing game" was popular. The sender clasped the receiver's hand or made tactual contact with some part of the body. The receiver then attempted to "read the sender's mind." Sugden (113) gave one of the earliest critical accounts of the successes produced by involuntary movements and suggested that telepathy experiments should be controlled by eliminating tactual contact.

Another variation of the contact cue was investigated by Stratton (105). Stratton's subject, a professional "thought-reader," made a specialty of finding hidden articles. He could be thoroughly blindfolded but a necessary condition for success was found in his use of involuntary kinesthetic cues obtained from a "leader." Another necessary condition for success was found in that the "leader" himself had to know where the object was hidden. The performer was so highly trained at using these involuntary kinesthetic cues that contact through a thin watch chain sufficed to produce remarkable "hits."

When the receiver is allowed to see the sender, involuntary movements or signals may be visually apprehended. Apparently these cues were used by "Clever Hans," the mind-reading horse investigated by Pfungst (76). The visual apprehension of involuntary movements was also proposed by Münsterberg (71) to account for the telepathic feats of Beulah Miller. Her successes decreased to the chance level when the family was outside the room. Some suggestion of the visual use of involuntary muscular cues is to be found in early reports of experiments with the Creery sisters by Barrett, Gurney, and Myers (5). It was noted that the successes of the sisters increased if the father was included in the group of senders and the girls were able to see him. Confessions of fraud (41) further invalidate these experiments, although at one time they were considered to be unimpeachable evidence by members of the British Society for Psychical Research. Button (16, 17, 18) has reported high scores in card guessing with the medium "Margery" as subject. Since, in the majority of the card tests, the sender was in full view

of the guesser, the question of involuntary cues appears to be of relevance. It is generally agreed at present that experimental methods which allow contact and use of involuntary muscular cues through vision cannot provide serious evidence for any ability beyond the learned skill in using minimal and subliminal cues to aid guessing.

Probably the best example of the use of tactual cues may be found in the clairvoyance experiment reported by Verall (117). She was able to obtain differential tactual cues from feeling the faces of playing cards as she was guessing their denomination. Feeling the backs did not produce positive results.

(b) *Visual Cues.* As stated above, visual observation of involuntary movement may serve to produce positive results in telepathy experiments. Verall (117) found that the faces of playing cards could be read with ease if they were dealt out over a polished surface or even over a white linen tablecloth. Coover (22) obtained results on the use of subliminal (*i.e.* nonverbalized) visual cues which definitely indicate that subjects may unconsciously use these cues to produce extra-chance results in guessing experiments and cited other experiments in which similar results were found. Since the cues may operate below the level of awareness of the subject, every possible use of visual cues should be eliminated in telepathy and clairvoyance experiments, even though the possibility of using such cues appears to be negligible.

(c) *Auditory Cues.* The error of unconscious or involuntary "whispering" cues in telepathy experiments has received little attention since 1895 when Hansen and Lehmann (45) advanced this hypothesis to explain results obtained by the Sidgwick and G. A. Smith (99), members of the British Society for Psychical Research. In the Sidgwick-Smith experiment, extra-chance scores were obtained by hypnotized receivers only so long as the sender was in the same room. Recalculations with modern statistical methods of the results obtained when the sender was outside the room indicate that these scores do not deviate significantly from the chance level. Hansen and Lehmann assumed that hypnotism produced auditory hyperesthesia in the receiver and that the sender, in concentrating, unconsciously whispered enough of the material which he was attempting to "transmit" to raise the total hit score above chance expectancy. In trying to reproduce the conditions of the Sidgwick-Smith experiment, Hansen and Lehmann used 2 parabolic sound reflectors arranged in such a way that involuntary noises made by the agent at the focus of one reflector were reflected and gathered at the focus of the other reflector where the receiver was stationed. Under con-



ditions in which the sender was aware of the purpose of the experiment but reduced his whispering to minimal intensity, scores significantly above the chance level were obtained. Hansen and Lehmann also showed that various positions in a closed room were not equally good for hearing small noises because of the nature of the sound reflection. An analysis of most frequent confusions in the Hansen-Lehmann and the Sidgwick-Smith experiments seemed to indicate to Hansen and Lehmann that errors in both experiments were produced by the same cause: namely, the misinterpretation of reduced and ambiguous sound cues. Sidgwick (98), however, pointed out that a good agreement was found between the modal errors in the Hansen-Lehmann experiment and in the trials from the Sidgwick-Smith experiment in which the agent was in another room. Sidgwick also criticized the Hansen-Lehmann work because the whispering was not "involuntary." Kennedy (60) has recently repeated the Hansen-Lehmann experiment with naïve senders (blindfolded and unaware of the action of the reflectors) and has found confirmatory evidence for involuntary auditory cues as a source of error in telepathy experiments.

In an unpublished manuscript answering the critics of card-guessing experiments reported in his 1917 monograph, Coover (23) claims that the small positive excesses may have been due to the subject's use of involuntary sound cues supplied by the agent. No graded effect of the distance was found between agent and percipient (in the same room) in 847 trials (p. 77), with distances of 1, 2, 3, 4.6, 6, and 10 meters. However, the factor of unconscious whispering in a few agents might have given slight positive deviations when the scores were summed for the total group of 10,000 guesses. Coover's experimental methods were uncontrolled for this source of error.

Finally, in evaluating experiments on telepathy and clairvoyance, due consideration should be given to opportunity for conscious fraud in the experimental conditions. The sequel of the British Society's experiments with the Creery sisters was an exposé of conscious signalling (41). The striking successes in the famous Smith-Blackburn series of telepathic experiments (42, 43) were attributed by Blackburn (12), 30 years later, to ingenious codes which escaped detection at the time.

## 2. "Mental Habits" and Preferences.

In much of the early work on telepathy, the material to be transferred by thought consisted of simple geometric drawings, familiar

objects and incidents. The question soon arose as to how much of the positive evidence for telepathy was attributable to communality of experience and preferences for certain diagrams and orders of diagrams. Pickering (77) and Minot (69, 70), members of the American Society for Psychical Research, conducted experiments relating to mental habits and found that percipients in thought-transference situations tended to call numbers and draw diagrams in accordance with their habitual preferences.

The use of simple diagrams and pictures as suitable material for telepathy experiments has also been questioned since the judgment of a "hit" may be dependent upon the experimenter's bias. Minot (70), Hall (44), and Hansen and Lehmann (45) pointed out the unsatisfactory character of such judgments in the early work on telepathy. Sudre (112) championed the drawing method and was answered by Bird (9), who proposed that in future work the material to be transferred should be simple enough to make possible the application of probability statistics. More recently, the series of telepathy experiments reported by the Sinclairs (100), in which simple drawings were used, have revived this issue. Prince (81), in a review of the Sinclair experiments, failed to consider the preference factor in discussing the results. Warcollier's (118) experimental methods were criticized by Soal (101) because the material would not allow exact probability estimations. The preference effects in these experiments arose when the agent was allowed to choose freely or select "mentally" the material to be sent. At present, there appears to be general agreement among experimenters that the material used in telepathy experiments should be chosen for the agent by a method which will insure a random series. Coover's experiments (22) illustrate methods and materials in which preference factors were successfully eliminated. Cason (20), Troland (115), and Estabrooks (30) also conducted experiments on telepathic ability in which mental habits and preferences could not produce spurious results. Estabrooks was the only one of this group to obtain extra-chance results in card guessing.

### 3. Recording Errors.

Another important factor in the production of statistical evidence supporting the telepathy and clairvoyance hypotheses has been the lack of control of the human element in recording the results of experiments. Unnoticed errors may occur in the records of individuals who are seeking evidence in support of their beliefs. The

necessary control of these errors in recording lies in making independent records of the guesses and the material to be guessed. The recorder should not have knowledge of the correctness or incorrectness of the guess. As will be shown later, this apparently insignificant point of methodology may have many ramifications in explaining positive results in telepathy and clairvoyance experiments.

In the experiments of the Sidgwicks and G. A. Smith (99), cited above in connection with the error of involuntary auditory cues, it appears that independent records of guesses and material guessed were not made when the sender and hypnotized receiver were in the same room, *i.e.* under the conditions in which the extra-chance excesses were obtained. Exact details of the recording procedure are not given but it is stated that Mrs. Sidgwick directed and arranged the material to be guessed and did the recording while the principals were in the same room. It may be assumed that independent records of guesses and numbers guessed were made when subject and operator were in different noncommunicating rooms. To complete this error theory, the assumption is further made that the recorder knew what the correct responses should be and may have made unnoticed errors which automatically increased the score above the chance level. Internal evidence that errors in recording were made is to be found in the Sidgwick-Smith paper (Table III, p. 153). Only numbers from 10 to 90 were used in the experiment, yet 91 appears as one of the numbers on which the agent concentrated.

Experimental conditions in the researches of Brugmans (14, 15), in collaboration with Heymans and Weinberg, in which positive results in favor of the telepathic hypothesis were reported, apparently did not exclude the factor of unnoticed errors in recording. The recorder apparently looked through a hole cut in the floor of an upper room and observed the subject making his choices manually by indicating a certain square on a board. If the assumption is made that the recorder knew what the correct choices were, these results are explainable on an error basis. Murphy (73) states:

"Curiously enough, 40% of the experiments between the two rooms were complete successes while only 30% of those in the same room were successful."

If the further assumption is made that peering through a hole at the subject's hand does not constitute a favorable condition for exact

observation, this may also have an explanation<sup>4</sup> in terms of recording errors. In an earlier experiment from the Groningen laboratory by Van Loon and Weinberg (116), in which emotions, colors, and thermal sensations were used in attempted thought-transference, the criticism of nonindependent recording also applies as do those of kinesthetic, auditory, and visual cues, and preference effects.

Jephson (52) reported positive results in clairvoyance experiments in which the subject made his own record of both the actual and guessed card orders. Later experiments by the same writer (53) and with Soal and Besterman (54), in which recording was carried out independently, yielded results at the chance level. A telepathy experiment by Besterman (8), in which recording was done independently, yielded negative results for the telepathic hypothesis.

Methods of recording are important to consider in attempting to understand experiments in which results in favor of the telepathic and clairvoyant hypotheses were obtained. In the majority of past experiments, the condition of independent recording reduced the successes to the level expected by chance. An exception to this general rule may be found in Estabrooks' telepathy experiment performed at Harvard University. Many experiments for telepathy and clairvoyance are unsatisfactory because of a combination of such uncontrolled methods; for example, the Sidgwick-Smith series did not eliminate either involuntary auditory cues or unnoticed errors in recording. Coover (22), Cason (20), and Troland (115) have, in the past, suggested that independent recording should be a necessary condition for acceptable scientific methodology in experimentation on telepathy and clairvoyance. The specific ways in which knowledge of correct guesses at the time of recording may produce unnoticed errors will be discussed more fully in connection with the Duke experiments on ESP.

#### B. STATISTICAL METHODS USED IN PSYCHICAL RESEARCH BEFORE ESP

##### 1. *The Limit of Chance Variation.*

The statistical methods in research on telepathy and clairvoyance have been subjected to criticism since their introduction by Richet (91) in 1884 and their further elaboration by Coover (22) in 1917. Sanger (94) and Edgeworth (27, 28) concluded that the

<sup>4</sup> Murphy (73, 74) has twice written reviews of this experiment without fully stating the types of control exercised beyond guarding against sensory cues. Since the present writer has not been able to obtain the first report, decision as to the probable correctness of the error hypothesis depends upon a further knowledge of the experiment.

application of theoretical probability formulae to the description of the chance or extra-chance nature of results obtained in simple guessing experiments was justified. Probably the most important criticism concerning statistical methods in pre-ESP telepathy and clairvoyance experiments was raised by Coover's selection of the limit of chance expectancy (22). Thouless (114) has stated that Coover's limit was too high by a factor of the square root of 2. In explaining his selection of  $L = \sqrt{2} \cdot 3\sigma$  as the limit of chance variation, Coover, in a footnote (p. 85), stated that he was following Sanger's (94) formula,  $K = 3\sqrt{2(1-q)mq}$  [ $q$ =probability of occurrence,  $m$ =number of trials], which reduces by appropriate substitutions, to the formula used by Coover.

## 2. Selection of Data.

When evidence for telepathy and clairvoyance is obtained by comparing frequencies of correct guesses with frequencies expected by chance, one of the more obvious errors in experimental method arises from selection of favorable data for statistical analysis. In the hands of inexperienced experimenters, such selection has in the past produced spurious statistical evidence for the existence of telepathy and clairvoyance. The importance of this source of experimental error led Soal (102) to make the following statement:

"Moreover, instead of being given an exact account of the precise conditions under which each experiment is carried out, we are regaled with 'samples' of successes generally chosen to illustrate preconceived theories of the way in which telepathy is supposed to work. I believe this inveterate determination to find the super-normal at all costs in every series of experiments to be the most injurious influence in psychical research today. The scepticism of the open-minded man of science who has not given much attention to the subject is an entirely wholesome thing compared with the 'will to believe' shown by the class of psychical researchers whose real aim appears to be not the investigation of the conditions under which telepathy and clairvoyance occur, or the question whether they occur at all, but the *production of examples of these faculties for the purpose of bolstering up beliefs they hold on the destiny and spiritual nature of man or for the purpose of confuting the spirit hypothesis*. It is easy to understand how the psychic researcher actuated by such motives becomes the prey to self-deception . . . His desire is for super-normal happenings, and he is not disposed to look too critically at the methods by which these supposedly super-normal facts were established. And experiments in telepathy and clairvoyance unfortunately are the easiest in the world to manipulate so as to give the illusion of success" (pp. 271-272).

In an unpublished manuscript, Coover (23) points out that critics, especially Schiller (95), who have "recalculated" the results



in his 1917 monograph, have obtained their "evidence for lucidity" by selecting scores from a total distribution of trials which exceeded the mean chance expectancy. To quote from this manuscript:

"Selection for separate treatment should not be made from the figures in the distribution; it should be made from the conditions of the experiment. Or, if selection is made on the basis of figures in the distribution, it should be for the purpose of going back to the data to seek for conditions that may be responsible for the peculiar nature of these figures, rather than for putting them through a statistical mill that is no more applicable to them."

Again:

"All one needs to do is to select sufficient data removed well from the mean of the distribution and he can 'prove' the presence of any cause he has an interest in promoting."

To prove the Schiller hypothesis of "lucidity in selected cases," consistency measurements on the scores of the selected group would be necessary. Coover did not provide the necessary retest data for the determination of consistency.

In order to summarize briefly this discussion of sources of error found in experiments on telepathy and clairvoyance before ESP, errors and suggested controls may be arranged in the following table:

TABLE I

SOURCES OF ERROR IN PRE-ESP TELEPATHY AND CLAIRVOYANCE EXPERIMENTS

Errors	Controls
1. Minimal and subliminal sensory cues a. Kinesthetic and tactual b. Auditory c. Visual	No contact, preferably distance separating agent and percipient. Vision and audition excluded by suitable tested methods.
2. Mental habits and preferences	Use of materials, such as playing cards, with which the probability of success in guessing may be determined. Use of methods for "randomization" of material to be guessed, such as a thorough mechanical shuffle of a deck of cards.
3. Unnoticed errors in recording original data	Independent records. Recording done without knowledge of success or failure.
4. Selection of favorable data	Inclusion of all data obtained under same conditions for statistical analysis. Decision as to conditions favorable to occurrence of telepathy and clairvoyance before rather than after collection of runs.

## III. EXPERIMENTAL METHODS IN ESP RESEARCH

## A. METHODOLOGICAL ERRORS

The majority of the modern experiments purporting to demonstrate the existence of ESP can be reviewed profitably with respect to the sources of error discussed in the previous section. In the following discussion of the ESP experimental methods, these techniques will be presented by the device of direct quotation from the major publications of the Parapsychology Laboratory and from experimental articles in which positive results obtained by these methods have been accepted as evidence for ESP.<sup>5</sup>

1. *Unnoticed Sensory Cues.*

(a) *Visual Cues.* Three of the methods recommended by Rhine and others are open to the criticism of unconscious use of small visual cues on the part of the ESP subjects. These methods are (1) the Open Matching test, (2) the Blind Matching test, and (3) the Single Card Calling, or Before Touching test.

In the "Handbook," the Open Matching test (OM) is described as follows:

"*Getting ready.* Select five cards, one of each kind, from an extra pack of ESP cards. Place these, as 'key cards,' face up in a row in the middle of the table before you. Lay the rest of the pack aside.

"*Thoroughly shuffle another pack of twenty-five ESP cards, and then cut it. Hold it face down in your hand as though you were going to deal the cards.*"

"*Matching.* Now try to decide which key card the top card of the pack matches. Choose your strongest hunch or clearest impression. (Follow the general directions given for Test I.) When you have decided, remove the top card without looking at it and place it face down in front of that key card.

"*Do the same for the second card in the pack, then the third, and so on until all twenty-five cards have been put down where you think they match the key cards.*"

<sup>5</sup> Although *A handbook for testing extra-sensory perception*, written by Stuart and Pratt (111), is misleading as to the details of recording in some experimental methods, as will be pointed out later, it will be used as the main source for the general features of Rhine's experimental techniques. In general, the Rhine methodology will be quoted from (1) *A handbook for testing extra-sensory perception*, hereafter referred to as the "Handbook"; (2) Rhine's monograph, *Extra-sensory perception*, hereafter referred to as the "Monograph" [Rhine, 83]; and (3) experimental articles in the *Journal of Parapsychology*, edited by McDougall and Rhine.

"Place each card as though it were the only one, without regard to where you have put the others. Avoid falling into any system in placing your cards, such as laying them down in order from left to right. You may occasionally choose the same key card twice or three times in succession. Look through a shuffled pack to get an idea of the random order in which cards come. You do not need to keep an even number in each pile nor to have even piles at the end of the run.

"*Checking up.* When you have finished the run, find your score by counting the cards in each pile which match the key card before which they lie. Write the score for the run into one of the spaces of a record sheet.

"*Recording.* If you wish to keep a record of all the cards, list them on the record sheet in the following manner. Record the left-hand key card in the first 'call' space. Record the cards which were placed to match it in the 'card' column. Draw a line under the last card. Then record the other key cards and their corresponding cards in the same manner, separating each pile from the next by a line" (pp. 18-19).

Experience with this method shows that it is practically impossible for the subject to match cards without looking at the backs in the process. A photograph, purporting to illustrate the Open Matching method, may be found opposite p. 246 in Rhine's book (87), *New frontiers of the mind*. This photograph shows the subject with the card to be matched in her right hand, looking at the back of the card.

The backs of the cards seem to be available to the subject's vision in the Blind Matching test (BM). Directions for this test in the "Handbook" are as follows;

"This test is like the Open Matching Test except that here the key cards are face down and their order is unknown.

"*Getting ready.* Shuffle the five key cards out of sight so that you do not know their order, and place them face down in a row on the table.

"*Matching.* Take the shuffled pack of ESP cards face down in your hands ready to deal. Try to get a feeling of where the top card should go in order to match a key card. In this test it does not help to attempt to guess the symbols on the key cards. Just place the cards as they seem to belong together. When you have decided where to put the first card, place it down without looking at the face and go on to the next. Keep on until each card has been placed.

"*Checking up.* First turn the key cards face up, then count the number of cards in each pile correctly placed to match the key card. Record your score on one of the record sheets. Or list all the cards as described for Test III on p. 19" (pp. 22-23).

Again, the criticism of possible visual cues from the backs of the cards applies to the Single Card Calling, or Before Touching (BT) test, described in the "Handbook" as follows:

*"Getting ready.* Place a well-shuffled pack of ESP cards on the table before you. Cut the pack, taking care not to see any of the faces of the cards. The table at which you are working should not have a shiny surface that might reflect an image of a card face.

*"Calling and recording.* You are now ready to attempt to call the top card, thus testing your ESP of objects. Do this in the same easy, natural manner suggested for making a call in Test I. . .

"When you have made your choice for the top card, write it on the record sheet, remove the card, and place it on the table without looking at the face. Now decide in the same way what the second card is, record your call, and remove the card, placing it face down upon the first one. Do the same for the third card, the fourth, and so on until all twenty-five have been called.

"As in Test I, you may not feel sure of your calls as you make them. Go right ahead, doing your best and waiting until the results are checked to learn of your success.

*"Checking up.* When the run is completed, turn the cards over and record the actual order beside the calls. Mark the hits clearly, and put the score at the foot of the column" (pp. 15-16).

Experiments using these methods in which positive results for the clairvoyance hypothesis have been reported are those of (1) Rhine (83), with the BT method checked up after 5 calls and after 25 calls; (2) Woodruff and George (128), in which the OM and BM tests were introduced and the BT method was also used; (3) Pegram (75), with BT and OM tests; (4) L. E. Rhine (90), with OM; (5) Gibson (33), with OM, BM, and BT methods; (6) Humphrey and Clark (48), who used the BT method; (7) "Anonymous Scientist" (2), with the BT method; (8) Pratt and Price (79), using the BM method; and Warner and Raible (122), with OM and BM. Evidence that some of the cards actually do afford visual cues under certain conditions may be cited.

Three kinds of cards have been used in the ESP experiments. In the early work, reported in the "Monograph," experiments were performed with Zener cards and an earlier version of the present ESP cards. In "Letters and Notes" (64), the cards are described as follows:

"In the beginning of ESP research at Duke University, when it was assumed as a working principle that the subject should, if he wished, be able to locate directly the card he was calling, the cards used were cut

(mostly die-cut) from heavy cardboard, were opaque to a 100 watt light, were hand-stamped with ink that left no tactual impression or warping, and were carefully inspected for secondary cues on the backs" (p. 72).

These were the Zener cards. The earlier version of the ESP cards, used in the majority of the "Monograph" experiments, were stamped with a rubber stamp on blank playing-card stock which had a filigree design on the back, as shown in the photograph on p. 48 in *New frontiers of the mind*. The commercial ESP cards are printed with heavy symbols on the faces and a design of white lines on the backs, which are otherwise solid blue.

Soon after the commercial cards were made publicly available, it was discovered that under certain conditions of lighting the symbols could be read from the backs of the cards. Gulliksen (40) made the further observation that these cues might be used unconsciously by a subject who was allowed to look at the backs of the commercial cards while matching them in the OM and BM methods and calling them in the BT methods. Kennedy (59) presented a case in which significant extra-chance scores were made by a subject who was allowed to look at the backs of the cards in the OM test. This subject reported that he was unaware of using cues on the backs of the cards. In the same paper, a photograph of the cues taken under optimal lighting conditions is also presented. Small marks put on the cards by shuffling and handling may also serve to raise the guessing level significantly above that expected by chance. A subject in the Stanford laboratory made high scores with the BT method and the earlier version of the ESP cards when the same deck was used over a long series and vision of the backs was permitted. She was unable to verbalize concerning the nature of the cues but examination of the cards showed small marks which might have been associated with the correct symbol unconsciously during the checking-up process. Because of the possibility of using small visual cues in the methods described above, the conclusion may be made that extra-chance results obtained by these methods do not satisfy scientific requirements for evidence in favor of ESP.

(b) *Auditory Cues.* The question of involuntary auditory cues arises in connection with several experiments reported in the modern ESP literature. Warner and Raible (121), in threshold judgments of weight discrimination, obtained extra-chance results when the experimenter knew the correct choice and chance results when the position of the correct choice was unknown. The writers state:

"Critical consideration of the conditions leads us to believe all modalities beyond suspicion except hearing. . . True, it seems far fetched to



suppose that such cues could in any way be construed so as to aid in the making of correct judgments. It is also true that our subjects (and there seems no reason to doubt their word) were entirely unaware of any assistance from such sources or from any sources other than the feeling of the weights themselves. . . . For this reason, further discrimination tests are being made. . . . The experimenter and the subject, instead of being separated by a mere screen, are confined in non-communicating rooms" (p. 50).

Until further crucial tests are made, as suggested in the quotation, these results are made equivocal as evidence for the telepathic hypothesis by the possibility of minute sound cues. Bender (7), in a report on the alleged telepathic ability of a feeble-minded girl, concluded that involuntary auditory cues were used by the subject in performing her unusual feats. Drake (26) also has worked with a feeble-minded case which presents striking similarities to the one reported by Bender. Unfortunately, the Drake investigation, as reported, was not carried to the crucial stage of elimination of all possibilities for the use of cues. The experimenter writes:

"We hope it will be possible to continue the investigation using only rigid control conditions" (p. 195).

As suggested in the Warner-Raible quotation, rigid control conditions for auditory cues would involve placing sender and receiver in different sound-shielded rooms.

## 2. "Mental Habits" and Preferences.

The Pure Telepathy (PT) method in ESP research is described as follows in the "Handbook":

*"Getting ready.* This is a test for the ESP of a thought alone. No cards are used for the sender to look at. He simply chooses a random order of the ESP symbols and holds these in mind (without writing them down) until the receiver has made his choices. Success in this test depends entirely upon reading the sender's thoughts.

"When everything is ready for the test to begin, the sender decides what the first five symbols will be. He imagines an order of five ESP cards as they might come in a shuffled deck. Then he concentrates upon the first symbol, and signals to the receiver that he is ready.

*"Calling and recording.* The receiver sits where he cannot see the sender. When he gets the first signal he makes and records his call and signals back to the sender. The signals may be given by tapping with a pencil, as before. After the receiver has signaled that he has made his call, the sender writes down what symbol he had in mind.

"The sender then concentrates on the second symbol of the five, which has already been chosen, and gives the signal. The receiver records his call for this trial and signals back. When the first group of

five symbols has been used, the sender chooses another order of five and concentrates on the first one of these. This goes on until the run of twenty-five trials is completed" (p. 50).

Mental habits and preferences may enter into the agent's selection of symbols on which to concentrate during this test. Kellogg (56) has criticized the PT method as follows:

"Again, if experimenter and subject happen to have thought preferences in favor of the same one or two of the designs, scores will be increased regardless of any mutual influence. Any experimenter in such work will tend to form more or less definite order habits. This possibility was recognized [by Rhine], and precautions taken to counteract it. Cross-checks, applied to *other* series, show these precautions were on the whole successful. At any rate, habits did not become so established that similar plans were used in *immediate* succession. But many of the different arrangements of the designs, that would occur in the shuffling of the pack, would almost certainly not be included among the plans chosen consciously. Some might seem too systematic, e.g. aaaaa bbbbb, etc.; aaaaa bcd bcd, etc., . . . eeeee. On the other hand, others with no apparent plan would give no ready means of keeping to the equal frequencies desired. If such helter-skelter arrangements are avoided, the range of possibilities is reduced; if not, there is more scope for symbol preferences which may be correlated with those of the subject. Rhine, as a means of controlling frequencies, coached his experimenters to plan by groups of five, which leads most naturally to five sets of one each of the five designs, thus very much reducing the number of different arrangements. (From 623 trillions down to less than 25 billions, subject to further reduction by avoidances, etc.) This extreme he seems to have tried to prevent, but the measures taken for the purpose are not such as to result in anything like the total possibilities of shuffling. There has been no mention of any attempt to keep secret the instructions given for the conduct of the tests. It would be strange if students interested in the work did not sometimes discuss various possible arrangements of the symbols. However that may be, subjects would surely tend to follow somewhat the same general sort of procedure as the experimenters—which means a marked increase in the chances of high scores. As the subjects were informed of their success at the ends of the runs, adoption of plans of the type used by the particular experimenter would be favored. Skill could thus be developed just as it is in various familiar card games. Indeed, a good player might adjust to this experimental situation almost immediately" (p. 340).

Rhine's method (84) for determining whether or not preferences in choosing symbols helped to produce extra-chance scores was as follows:

"The question of similarity in habits of choice may be further checked by cross-checking the agent's record with the percipient's of—let us say—the day preceding or following, that is, correlating runs not intended to

match. A cross-check was made of the Junaluska Series given below and it yielded an average of only 4 hits per 25. Habitual similarity of order of choices between agent and percipient can, I think, be said to have been satisfactorily eliminated. Moreover, in the series cross-checked as just described, the runs were made daily and the cross-check, therefore, covers also the question of daily routines or patterns of order of choosing.

"Finally, as an extreme test of 'patterning' or order habits, we may check the percipient's records against themselves in consecutive order. The percipient might have a pattern of order without its being coincident with a similar one supposed for the agent. But if *he* does *not* have such a pattern, the question of similar order-habits is ruled out. Such a check made on 12 of the percipient's records given below, taken consecutively from a block chosen at random, and on the 8 records of the Junaluska records, yields an average of coincidences of 4.6 between runs, as against an expected 5" (pp. 223-224).

As Kellogg points out, preference effects may involve such simple errors as the tendency to choose a different symbol for the following choice. Since apparently not all of the data have been checked for these factors and since there is some question as to the adequacy of the methods used to prove the absence of preferences and order habits, the status of extra-chance results obtained by this method as scientific evidence for telepathy is also open to question.

Perhaps the best large-scale example of the working of preferences to produce unacceptable results with respect to the telepathic hypothesis may be found in Goodfellow's report (34) of the recent Zenith Foundation tests for mass telepathy.<sup>6</sup> Fernberger (32) has also indicated the presence of preference effects in the Zenith experimental methods.

Willoughby (125), Wolfe (127), and Gulliksen (40) have discussed another source of error due to possible "rational inference" in the BT method when the subject is told his success or failure after each guess or after each 5 guesses. Wolfe states:

"If after each call or after each five calls the subjects are allowed to see the cards which they have called, they may infer rationally which

<sup>6</sup> For results of previous broadcasting tests for mass telepathy, see Woolley (129) and Bird (9). Goodfellow's work has not yet been published in detail. Preference factors arose in the Zenith results because 2 alternatives and but 5 guesses per session were used. Under these conditions, the guessers had only 32 possible patterns (such as 1,1,2,1,2 or 2,1,2,1,2) from which to choose. Although all 32 patterns were equally likely to be chosen by chance as correct, the frequency rankings showed that the audience preferred some patterns over others, thus piling up more large and small deviations from chance expectancy than would be predicted by theoretical probability statistics.

suits are left in the deck in the greatest frequency and so increase their average score above five. Willoughby and Wolffe have pointed out this source of error and have obtained average scores of 7-9 with the use of rational inference but without (consciously at least) using any extra-sensory powers. If Rhine also used these methods some of his above-chance scoring is easily accounted for" (p. 949).

The  $BT_1$  and  $BT_6$  methods have not appeared in published reports since the publication of the "Monograph."

### 3. *Errors in Recording.*

The task of the reviewer in the present section will be to establish the essential validity of 2 propositions: (1) The methods of recording the original data in the majority of ESP experiments do not eliminate the possibility of unnoticed errors in recording, and (2) the experimental conditions under which the ESP to be discussed in this section appears at its height are also those in which unnoticed errors in recording may be expected to occur. In the final analysis, of course, the validity of the first proposition rests on repetitions of the Rhine experiments with the possibility of checking on the accuracy of recording. Evaluation of the experimental conditions which favor the appearance of extra-chance scores will depend upon the findings of experimental psychologists on the determiners of attention and suggestibility.

(a) *Clairvoyance Methods.* Descriptions of the recording procedures utilized in the Open Matching, Blind Matching, and Single Card Calling methods may be found above in the discussion of the possibilities of the use of sensory cues. It should be noted that the recorder, or the person who decides how many hits were scored, knows in the matching methods which cards should have been matched to the key cards to yield high scores. In the Single Card Calling method the subject's guesses are recorded first, then the actual symbol is recorded in a small space directly opposite the called symbol. Sometimes, the subject calls aloud the correct order of the symbols to the recorder, sometimes the recorder leafs through the pack himself to make the check-up. "Hits" are circled after the records have been made, not at the time the correspondence is called. In many cases the number of hits is recorded and no record is made of the cards matched to the different symbols. A partial check on the accuracy of recording would involve counting the symbols to see if the necessary 5,5,5,5,5 frequency was present. When all the symbols are not recorded, it is impossible to apply this check.

The Screened Touch Matching method (STM), in which every possibility of sensory cues seems to be eliminated, appears to be open to the criticism of unnoticed errors in recording. The method is described in the "Handbook" as follows:

"This is a screened matching test in which you show where you want the cards placed by touching the key cards one after another while your co-worker holds the cards out of sight and places them into five piles as the choices are indicated. In the picture facing p. 34, the person on the right is taking Test VI.

"*Screen.* The screen is an upright board twenty-four inches square. At the bottom of the screen is an opening two inches high and twelve inches long. The key cards are to be placed in a row in this opening. Three inches back of the opening, on the opposite side of the screen from you who are being tested, is an upright board a little longer and higher than the opening in the screen. While this backboard keeps you from being able to see the cards in the hands of your co-worker, it permits him to see the key cards as you touch them.

"The test is made like any other matching test, except that you show by touching one of the key cards with a pointer where you want the top card of the pack placed and let your companion put it down for you.

"*Getting ready.* Arrange the key cards in a row before the opening in the screen on the other side from the low backboard. Use the eraser end of a pencil or some other pointer to touch the cards.

"*Matching.* The co-worker sits on the other side of the screen, with the backboard between him and the key cards. He shuffles and cuts the pack out of your sight, and gives the signal when he is ready. He watches your pointer, through the opening in the screen, and as soon as it comes to rest on one of the key cards he takes the top card from the pack without looking at it and places it face down behind the backboard opposite the key card touched. Go right on touching key cards at your own speed. The cards can usually be put down fast enough to keep up with you.

"*Checking up.* When the run is completed, the person handling the cards counts the number of hits (cards correctly matched). The score is put down on the record sheet and the test continued through the usual four runs, or as many more as seem advisable" (pp. 27-28).

No check on recording errors appears to be possible with this method. The method would be improved if independent records of the matched cards were made by a person who does not know the key-card symbols at the time the recording is done.

Referring back to the description of recording methods in the Single Card Calling or Before Touching test, it is evident that the recorder has the record of the subject's calls before him while checking-up the card series. Unnoticed errors in recording may



arise due to split attention and expectancy of extra-chance deviations. The recorder, instead of writing the symbol he sees in the deck, may unconsciously record the symbol which he sees in the call series, thus automatically producing a hit which will be accepted when he goes back over the record to mark the correspondences between card and call series. As a partial control on these errors, records should be rechecked for variations from the 5,5,5,5,5 pattern of cards as suggested by Carpenter and Phalen (19). This check is not completely satisfactory, since errors in transposing the position of symbols may be made. Apparently no check on accuracy of recording is available for the transposition error. The practice at the Stanford laboratory has been to use a different deck for each set of 25 guesses, the order of symbols in which has been previously recorded without the subject's knowledge before the experimental period. When the "correct" order is applied to the set of symbols recorded at the time of experiment, the presence or absence of errors in recording may be determined and permanently recorded.

The Down Through or Pack Calling test (DT) is described in the "Handbook" as follows:

*"Getting ready.* Place a well-shuffled and cut pack of ESP cards on the table. Get ready to record your calls for the cards just as in Test II. Follow the directions for making your calls as in Test I.

*"Calling and recording.* When you are ready, simply proceed to make your calls, but without laying the cards off the pack as you call them. Try to get an impression of what the top card is and record this. Then try to get the second card down in the pack, then the third, and so on through the entire twenty-five. The test is to see how many more hits you can get reading straight down through the pack than would be expected by chance. In Test II each card was laid off the pack after it was called. In the present case, however, none of the cards are removed until all twenty-five have been called.

"It is not necessary to keep the position of each card in mind as you make each call. Say to yourself before starting the run that you are going to read the cards right down through the pack; then take the impressions of the symbols in the order that they come most easily and vividly to you. If you are writing your own calls, you will know that you should stop when you have put symbols in all of the spaces for calls. When an assistant records for you, he will tell you when to stop. It is not necessary for you to count your calls as you make them in order to succeed.

*"Checking up.* Check your results as usual by recording the actual order of the cards alongside your calls. Be careful to take the cards in the order in which you tried to name them, beginning at the top of the pack.

*"Going on with the test. Shuffle and cut the pack and make the second run . . ."* (pp. 21-22).

The same criticism of possible unnoticed errors in recording the card series applies to this test. As in the case of the Single Card Calling method, the check-up is accomplished either vocally or visually and the conditions of split attention still may be present.

The distance clairvoyance tests also appear to be liable to the criticism of unnoticed errors in recording. Rhine (85) states:

"In 26,125 trials conducted during the summer of 1937 by Mr. James MacFarland of Tarkio College, the comparison of score averages in relation to various distances indicates again that distance is not a limiting condition.

"The tests were made with the DT technique, the cards being kept intact in packs by the experimenter throughout the test period and removed only when checking up. The subjects filled out five columns of a record sheet, one for each pack in the experimenter's desk, at any time they wished on a given day, and sent the sheet to the experimenter to be checked. Double checking was carried out, and the general sponsorship of Dr. R. W. George, Head of the Department of Psychology, was exercised over this series. The cards were well shuffled and kept under careful observation by the experimenter" (p. 180).

In the Duke-San Diego series reported in the same article, specific descriptions of recording methods are not given. It may be assumed that the check-up was made with the subject's calls available to the recorder as in the usual DT experiments.

Recording in the "Campus Distance Series in Clairvoyance" is described by Rhine (86) as follows:

"The observer and subject synchronized their watches, and arranged to work at a stated time and distance. At the specified time the observer would take the top card from a shuffled pack of ESP cards in the room agreed on and lay it face down on a book in the center of the table without looking at its face. Thirty seconds later the subject in his cubicle in the Duke Library would record a call for the card. At the end of the minute, the observer would remove the card and take the next one. The cards as removed would be kept in order for later recording. Two runs were made per day. . .

"In Groups A-D, the records were sealed up after each sitting and delivered to me before subject and observer got together" (p. 287).

The acceptability of this series of extra-chance results with respect to recording errors depends upon when the check-up was made and whether or not it was done with knowledge of the subject's

calls. Apparently the general methods for clairvoyance research are open to question on the basis of recording errors.

Specific accounts of method in experimental articles on clairvoyance in the ESP literature may also be reviewed with respect to control of possible recording errors.

Rhine (83) used the BT, DT, and distance BT and DT tests in obtaining the "Monograph" results. Control of these methods has been described above. Pratt (78) was able to obtain extra-chance results with the Blind Screen Touch Matching method, in which the key cards were placed in boxes and their order was unknown both to the subject and to the experimenter until the check-up for hits. The check-up is described as follows:

"When the deck was sorted out, the screen was removed, the experimenter turned up the key cards and recorded their order, and the actual distribution of the failures and successes under each key symbol was recorded and checked by both the subject and the experimenter" (p. 15).

Woodruff and George (128) used the OM, SOM, BM, SBM, BT, and SBT, the latter being the screened variety of the former, with success in guessing above the chance level reported. Objections to the check-up by these methods are to be found above.

Carpenter and Phalen (19), with the BT and DT methods, made the following observation concerning the check-up for hits:

"The two experimenters were used in Series 7 after G [the best subject] had made an error in reading off the true distribution. The error was apparently due to his divided attention during the checking process. He would look both at the card and at the record sheet and in so doing called once or twice when observing the symbol on the record sheet rather than the card's symbol. This error was discovered early in the series and checks of records indicated that it possibly had not previously occurred" (p. 39).

Martin (67) added a variation to the DT method in that the subject recorded his own correct order of symbols. The method is described as follows:

"The experimenter shuffled the cards thoroughly, cut them and placed them on the table before her. Exceeding care was taken that the bottom card was visible to no one. The subject then recorded twenty-five guesses on a record blank. The experimenter then read the actual order of the cards to the subject who recorded them. The experimenter carefully watched the recording of each card and was often checked by a third person. . . . It should be noted that neither the subject nor the experimenter had any knowledge of the actual order of the pack until after all twenty-five guesses were made" (p. 186).

It should be noted that the subject had the order of calls before her while the experimenter was calling out the order of correct cards for recording.

Pegram (75), with the OM, BT, and DT methods, found that the direction of extra-chance scores could be controlled voluntarily by the subject. But the direction of unnoticed errors in recording may also be involuntarily controlled by the aim of the recorder, as may be shown experimentally.

Martin and Stribic (68) report the use of the DT test with the following method:

"The DT or 'down through' method was used exclusively. The experimenter shuffled and cut the pack, and placed it face down before her, taking care to avoid seeing the bottom card. The subject then recorded his guesses for the order of the pack. After the completion of each run, and in the presence of both subject and experimenter, the pack was checked with the guessed order" (p. 24).

In a report by Humphrey and Clark (48), the BT test was used with no description of recording method given. In the case of Pratt and Price's (79) use of the BM test, recording was carried out as follows:

"At the end of each run the score was obtained by the experimenters, who turned the key cards out face up upon the table and then sorted the 'hits' in each pile from the 'misses.' In addition to counting the hits as they were sorted, both experimenters recounted and the score was recorded immediately" (p. 88).

No check on the accuracy of recording is possible with this method.

Shulman (97) worked with psychotic subjects with the Screened Touch Matching test. His recording technique is given as follows:

"After all of the 25 cards had been guessed, they would be checked to see how many had been matched with the correct key card. . . The cards were checked after each run of 25 cards and the subject was told his score" (p. 98).

Sharp and Clark (96) reported extra-chance scores with the OM, BM, DT, STM, and General STM methods. The last method, a variation of STM, is described in the following way:

"In GSTM the procedure was to have one experimenter look at the cards (held out of sight under the inclined screen). The experimenter holding the cards did not know the location of the key cards, which were placed by a second experimenter and the subject" (p. 127).

The necessary controls for recording errors are not mentioned in this article.

Stuart (109) has found recently that subjects performed at an extra-chance level in the matching tests when they were allowed to match at their normal tapping rate. Matchings done at a rate other than the normal rate yielded chance results. In discussing the possibility of error commission in recording as a cause of the observed deviation Stuart states:

"The method of recording in the screened matching procedure was as follows: After the subject had finished the run there were, on the table before the experimenter, five piles of cards, each in front of the symbol the cards were intended by the subject to match. Each of these piles was then turned face up and separated into two groups, the cards which matched the key card in front of the pile, and those which did not. As the correct matchings were thus sorted out they were counted. When the sorting was finished the correct matchings lay face up directly adjacent to the key symbols, and the first counting could be checked at a glance" (p. 179).

Apparently complete records were not made, nor was the recording done independently.

An adequate sample of recording methods in articles in which unsatisfactory clairvoyance has been obtained has been presented. The general rule is suggested that recording be carried out independently in order that the criticism offered may be eliminated.

(b) *Telepathy Methods.* Two methods for studying telepathy have been used by Rhine and others who have repeated his experiments. Unfortunately, the recording methods given in the "Handbook" for these 2 tests do not correspond to methods actually used in experiments in which high positive deviations from chance expectancy were obtained. The Pure Telepathy test was described in the section on mental habits. The reader may see from the description given there that the criticism of nonindependent recording applies since the agent usually acts both as sender and recorder of the percipient's vocal calls. In "Some selected experiments in extra-sensory perception" (84), Rhine appends a footnote:

"The question has been raised as to whether the agent might not, through her strong interest in getting good results, be likely to mistake unclear enunciation by the percipient who is off in another room, and to give favorable interpretations of calls not clear. First, the names of the symbols are phonetically quite easily distinguishable; each has a different vowel sound. Second, audition was good with open doors between rooms. Third, the scores for this work with the agent and percipient in the same room are nearly as high as with the two separated" (p. 224).



Independent records could have been made by placing a recorder in the room with the percipient and having the agent record only the cards, the number of hits to be determined by juxtaposing the 2 records and noting the correspondences. As stated above, the "Junaluska" Pure Telepathy series, although apparently controlled for independence in recording, is not satisfactory from the "mental habit" aspect.

Kellogg (56) has also discussed the auditory illusion criticism of the Pure Telepathy test:

"The experimenter had to try to think of the various designs as nearly as possible five times each in a run, but with no written plan to follow, give buzzer signals to call the attention of the subject for each trial, record the responses as heard, or supposed to be heard, check them if correct. Judge the effect on the scores of the almost inevitable tendency, especially in a laboratory so full of faith in ESP, to give the subject the benefit of the doubt in any trial involving some difficulty in hearing or in memory, and the great likelihood of illusions in hearing, the hearing of one call as a different one, especially when the observer gets excited in the course of a good run, and so hears what is desired instead of what is really uttered, yet with not the slightest realization of the possibility of a mistake. The much greater ease in following a lecture or play in one's own language has often been explained as due not so much to difference in understanding what is actually heard, as to the readiness to fill in gaps in auditory stimulation and, as far as conscious experience goes, hear the whole. In such a case, expectation is guided by the context, in line with the familiar usages of the native tongue. In the telepathy tests, success may breed success, in the record, by creating such an attitude of optimism that the observer-experimenter will strongly tend to hear the name of the symbol he has just been 'sending'" (p. 340).

Coover's work (22) on sound assimilation also shows that:

"... the ear cannot be trusted to report correctly names or phrases, when the latter are spoken under such conditions as are deemed by the recipient satisfactory for communication yet which permit some degree of indistinctness . . ." (p. 407).

The second telepathy test, devised by Rhine, is the Telepathy Card or Undifferentiated ESP method. In the "Handbook" a description of the test is given as follows:

"Two people work together in this test. One person shuffles and cuts the pack of ESP cards and looks at the face of each card while you, who are taking the test, try to name it. For convenience, we will call the person looking at the card the 'sender' and you the 'receiver.'"

"*Getting ready.* Fill in the blanks at the top of one of the record sheets to be found in the ESP Record Pad. It is best to leave these sheets in the pad as part of your permanent record."

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"*Getting ready.* Fill in the blanks at the top of one of the record sheets to be found in the ESP Record Pad. It is best to leave these sheets in the pad as part of your permanent record."

"The sender shuffles thoroughly and cuts a pack of ESP cards. He holds the pack with the faces toward him so that he sees the bottom card. He concentrates on the symbol on the face and signals to you by tapping with a pencil when he is ready.

"Sit where you cannot see the faces of the cards. You may close your eyes or look off into space, or even look at the backs of the cards. You may relax bodily or sit at alert attention.

"*Calling and recording.* When you hear the signal try to get an impression of the card at which the sender is looking. Do this in your own most natural way. You simply want to give the correct name of the card. You cannot reason out or force the correct answer. Simply decide upon the one which comes to you most easily and vividly.

"When you have decided, write your choice for the top card into the first space for 'calls' on the record sheet. In recording, let O stand for circle, + for plus, L for square, ~ for waves, and ^ for star. As soon as you have finished, tap with your pencil to signal the sender that you are ready for the next. Do not tell him what your call was.

"The sender then removes the card at which he has been looking and places it face down on the table without saying what it was. He looks at the second card and signals again when he is ready. Make your call for the second card and signal that you are ready for the third. Continue in this way until the run of twenty-five cards is completed.

"Choose your own speed. From one to five minutes to the pack is a favorable speed after you have become familiar with the test.

"You may not have the feeling that you are right in your calls as you make them. Many do not. This feeling does not always go with success.

"You must not know your success or failure on any card during the time that the run is in progress, since this would permit you to keep track of the cards. It is better if there is no talking during the run, but of course it can do no harm to talk between runs.

"*Checking up.* When you have finished calling all twenty-five cards, both of you check the score by turning the pack over and recording the order of the cards in the column marked 'cards.' You scored a hit every time the call and the card are the same. Mark these clearly, and write the total score for the pack at the foot of the column" (pp. 11-13).

According to this method, the percipient makes his own independent record of the calls. The check-up is carried out according to the questionable method of recording cards with knowledge of the calls. In a photograph opposite p. 19 of the "Handbook," with the title, "The Telepathy Card Test," the agent has the record book and appears to be recording the subject's calls. An earlier version of the Telepathy Card test, one that corresponds with the illustration in the "Handbook," may be found on the "Instructions" card in the new ESP decks. To quote:

"*Telepathy-Card Test.* For two persons. One acts as sender, the other as receiver . . .

"When both are ready, sender picks up pack holding face toward him, concentrates on first card and taps with his pencil on the table. Receiver tries to call the card, calling either the *first* one that comes to mind or that one of which he gets the clearest image, as he prefers. Allow only one trial for each card. The sender records the call in the 'call' column of the record sheet . . .

"The cards are kept in order until the end of the pack, then turned over and recorded in the *card* column as they are turned."

Possibilities for errors in recording the percipient's calls under these conditions are numerous and the direction of the errors is "channelled" to produce high scores automatically. When the agent is concentrating on the symbol in the deck, a reaction tendency to record that symbol is set up. The symbol called by the subject usually has the "right of way" to the final response of recording. Lapses of attention, "automatisms," expectancy of high scores, and suggestibility may give the symbol on which the agent is concentrating the "right of way" to the recording response and it may be recorded instead of the called symbol. When the actual card symbol is recorded later alongside of this error, a hit is automatically scored. Thus, the Telepathy Card test, in the form which seems to have been used in experiments reporting positive results, is uncontrolled for unnoticed errors in recording both in the card series and in the call series.

Experimental articles in which the latter version of the Telepathy Card test has been used are: (1) Rhine's "Monograph" (83) [pp. 59, 66] and (2) Gibson's article (33) in which no specific description of method was given. Kubis and Rouke (62), working with twins, obtained essentially chance results with a modification of the GESP technique which involved independent recording. Two selected batches of trials, however, were thought to indicate extra-chance results. Bond (13) has reported extra-chance scores with a group of retarded children. Specific details as to recording methods are not given.

(c) *Precognition Methods.* Rhine (88) has recently adapted the DT, OM, and the STM methods to the study of precognition or prophecy. His description of method is given as follows:

"Most of the 15 series of tests reported in this paper are based on the calling-before-shuffling modification of the DT procedure, called pre-



cognitive DT or PDT . . . The calls are made and recorded and the pack of cards then shuffled (face down) by the experimenter and checked against the record of the calls. The amount and type of shuffling varied somewhat with different investigators but consisted mainly of the dovetailing method, and of at least two such shuffles.

"In making the calls in these PDT tests the subject either wrote down the symbols or called them orally to a recorder. This record in earlier series was checked against the pack by the experimenter (with the subject as witness) after the 'random rearrangement' shuffling was done. In later series a record of the card order was made and the checking done by comparison of call and card records, thus permitting later re-checking.

"The exceptions to the test procedure referred to above consisted in adaptation of simple matching techniques to the precognition problem. One of these involved the open matching (OM) procedure. In this modification (POM) the cards were laid face down before a set of five blank spaces instead of the key cards, the key cards to be supplied by chance after the target deck was dealt out. The selection of the key cards afforded the 'random rearrangement' since these were chosen by a specified routine procedure from a second pack which was shuffled by the experimenter after the target deck was dealt. The subject of course tried to match the cards dealt against the set of key cards that were yet to come.

"In a similar way the screened touch matching (STM) method was adapted to the precognition research (PSTM). The key cards were chosen as just described, and the subject indicated his calls or choices by pointing to one of five empty shallow boxes which *would be expected to have placed in it later* the card to match that which the experimenter had on top of the pack held behind the screen.

"In all but one series the experimenter did the checking with the subject also witnessing when this was not prevented by distance. But in the last two series, two witnesses were introduced during the shuffling, card recording, and the checking of correspondences in the call and card records" (p. 47).

Apparently, in all of these methods the recording was not done independently. Rhine states that a record of the card series was made but does not say whether or not this record was made without knowledge of the subject's calls.

In the second paper in the precognition series by Rhine, Smith, and Woodruff (89), the writers state that, with deliberate intent, subjects can shuffle a pack of ESP cards and match symbols with a similar deck shuffled by an experimenter or a recorded order of cards

unknown to the subject with significantly better than chance success. The procedure is as follows:

"Following the shuffling of the shuffler's pack, the experimenter and subject (when the subject was an adult) laid off the cards of the pack in the order of their occurrence, observing matching or failure to match, and recording the results in numbers of hits (i.e., correct correspondences). When children were investigated, the experimenter himself or the experimenter and an observer—not a subject—handled both packs of cards. When a recorded series of symbols were used instead of the target pack, the record of the shuffler's pack was taken on the standard record sheet and coincidences with the target series checked in the usual manner.

"The greater part of the more crucial subdivisions of the data of this report have been independently re-checked for errors. This applies to all of the work in which a sealed target pack was used, and to approximately half of the screened target pack subdivision. Two experimenters (not subjects) were present throughout the experimentation and checking of the data for a substantial portion of the tests which yielded the highest score averages" (p. 121).

Apparently, complete records were not made in some of the experiments and no check can be made on order inversions in recording the deck in the experiments in which a prepared order of cards was used as the "target." No amount of "independent" rechecking after the experiment will bring to light such errors. Checking for the 5,5,5,5,5 frequency of symbols in the card column would be of some aid. This is not specifically mentioned.

(d) *Evidence in Favor of the Unnoticed Error Hypothesis.* Two important papers have already appeared in the *Journal of Parapsychology* which seem to indicate the validity of the criticisms discussed here in attempting to find explanations for these ESP results. Gibson (33) compared various ESP methods as to efficacy in producing extra-chance results. He concluded:

"If the averages of all of the eleven subjects in each technique are ranked from highest to lowest, they are as follows: OM, GESP, BM, DT, GESP (long distance), STM" (p. 269).

In Table II the possible sources of error are listed with the methods. Good agreement between possible sources of error and experimentally demonstrated potency for producing extra-chance results is found.

The second paper, written by Greenwood (36), describes methods used in an empirical control series, the aim of which was to demon-

strate conclusively that the mean correspondence in matching shuffled ESP decks with 100 sets of calls is exactly 5. The calls were not made with the intent of matching any of the decks of cards. The following quotation seems to add further evidence for the error hypothesis:

"At the end of 7,000 runs a random sampling recheck of 200 runs netted two errors of omission, lowering the score by one in each case. It was therefore decided to recheck the whole 7,000 runs or 175,000 separate comparisons. Accordingly, the writer went down one group of results and one or two assistants from the Parapsychology laboratory checked

TABLE II

THE RELATION BETWEEN POTENCY FOR PRODUCING "GOOD" SCORES AND SOURCES OF ERROR IN SEVERAL ESP METHODS

Method	Errors
1. Open Matching (OM)	Backs of cards seen by subject. Both types of minimal visual cues can be used. Nonindependent recording.
2. General ESP or Telepathy Card Test (GESP)	Possible attentional and illusory errors in recording calls. Possibility of checking-up errors.
3. Blind Matching (BM)	Backs of cards still available to subject. More difficult because key cards are face down. Nonindependent recording.
4. Down Through or Pack-Calling Test (DT)	Possibility of checking-up errors in recording cards.
*5. General ESP (long distance) (GESP l.d.)	Possibility of checking-up errors in recording, although least likely in this method.
*6. Screened Touch Matching (STM)	Nonindependent recording.

\*The difference in scores obtained by the 2 methods is small and appears to be nonsignificant.

on another part. The procedure was to obtain the new result first and then compare it with the old record. All told, 81 mistakes were discovered, 72 of omission by one and 9 of additions to the score by one.

"From runs 7,001 to 20,000, the end of the series, the same general procedure was used with the exception that the assistant and writer made *independent* records of scores, working on different parts of the series. At intervals the two records were then compared for differences. If differences were encountered these particular runs were carefully rechecked and errors corrected. . .

"For the whole series there were 12 errors of addition of one hit to a run, one of addition of two hits, and 77 of omission of one hit in the author's record. The total was 90 errors lowering the score by 63 hits out of an expected 100,000 hits. Since an urgent consideration of this

series was the avoidance of errors on even the first going over, it would seem appropriate to stress the fact that with a little less rigor the errors would undoubtedly have been much more numerous. In particular, since omissions were the more common type of error, work which gives rise to a significant *negative* deviation should call for rechecking. This type of error would of course only serve to diminish a positive deviation and the misleading effect be at least a safe one" (pp. 140-141).

These precautions in recording and checking the data given above cannot be found in the great majority of experimental articles on ESP in which extra-chance results are obtained. MacFarland (66) has recently published a set of extra-chance results with the GESP and DT methods in which "the experimental set-up was designed to eliminate the possibility of sensory cues and recording errors." Sensory cues were eliminated by distance. Recording errors in the call column were eliminated by checking 2 independent records of the subject's calls. But the check-up of actual order of cards was conducted with the calls available to the recorder. Thus:

"Both experimenters then checked the two decks of cards against the doubly-recorded order of calls. In this way there was secured double witnessing of all checking" (p. 162).

This is an unsatisfactory condition for elimination of errors in the card column.

It has been sufficiently demonstrated that the Rhine methods may allow either positive or negative errors to be made at the time of recording the original data. No satisfactory check to determine whether or not errors were made has been reported. Apparently, recording in these ESP experiments may be manipulated to prove a preconceived hypothesis of the recorder without his awareness of the manipulation. This is not without precedent in the literature of experimental psychology when records were ambiguous and the recorder worked with a strong motivation to produce results favorable to an hypothesis.

#### 4. *The Laboratory Conditions for Producing the Majority of ESP Data.*

The second proposition to be established by reviewing the ESP literature is that the conditions under which extra-chance scores seem to be obtained are those which foster unnoticed errors in recording. These conditions in the recorder, as stated above, are expectancy of favorable results, split attention, excitement, and amenability to suggestion.

It is interesting to note that modern experimental psychology had its inception in the study of the "personal equation" of errors in observation due to divided attention. The articles of Münsterberg (72), Bauch (6), Crosland (25), and Kollarits (61), to mention but a few outstanding studies, have contributed to the knowledge of sources of error in human perception and the conditions under which errors are made.

In Rhine's (83) Appendix to Chapter 15 of the "Monograph," entitled "Suggestions to Those Who May Care to Repeat These Experiments," the conditions are given for the production of extra-chance scores with Rhine's experimental methods. These suggestions are specifically made to aid in the selection of subjects who may produce good results. To quote:

"It is hoped that others will repeat these experiments or, better still, perform more advanced ones. Much depends upon the conditions of the tests as to whether success or failure will follow. The following suggestions along with the discussion in Chapter 12, may help to avoid failures:

"1. The subject should have an active interest in the tests and be fairly free from strong bias or doubt. These would, of course, hinder effort and limit attention. An open-minded, experimental attitude is all that is required. Positive belief is naturally favorable but not necessary.

"2. The preliminary tests should be entered into very informally, without much serious discussion as to techniques, or explanations or precautions. The more ado over techniques, the more inhibition is likely; and the more there is of explanation, the more likely is introspection to interfere. Playful informality is most favorable.

"3. If possible to do so honestly, it is helpful to give encouragement for any little success but no extravagant praise is desirable, even over striking results. The point is that encouragement is helpful, apparently, but only if it does not lead to self-consciousness. If it does, it is quite ruinous. Many subjects begin well, become excited or self-conscious, and then do poorly. . .

"5. It is highly important to let the subject have his own way, without restraint, at first. Later, he can be persuaded to allow changes, after he has gained confidence and discovered his way to ESP functioning. Even then, it is better for him to have his way as far as experimental conditions can allow. It is a poor science that dictates conditions to Nature. It is a better one that follows up with its well-adapted controls and conditions.

"6. It is wise not to express doubts or regrets. Discouragement seems to damage the delicate function of ESP. Here again no doubt personalities differ. One subject, I know, has worked in the face of doubt expressed; but she is exceptional in this . . .



"12. It is best to try good friends for Pure Telepathy at first—or couples, single or married, who feel certain they have thought-transference; and, above all, to try those people who say they have had 'psychic' experiences or whose ancestors conspicuously have had.

"These are suggestions, not rules, for we do not yet know enough of the subject to lay down rules. They will help toward success, without endangering conclusions. One can always tighten up on conditions before drawing conclusions later. But any investigator must first of all get his phenomena to occur—or exhaust the reasonable possibilities in trying to" (pp. 166-168).

Attitudes of expectancy of good scores, "playful informality," and positive suggestibility in the subject seem best to help the unnoticed or unconscious use of sensory cues. But it is these conditions at work in the experimenter or recorder which seem to be most important in attempting to understand the production of ESP. It should be noted further that the encouragement of "playful informality" by the experimenter involves the condition of split attention which is a favorable if not an absolutely necessary condition for unconscious error production.

Excitement during experiments appears to be common. A sample may be taken from Rhine's (87) description of conditions in which a subject made 21 hits out of 25 calls:

"For some time we drove along quietly. Then it occurred to me to test my subject on the way to the place where I had planned to make our first stop. I pulled the car up at the side of the road but did not bother to turn off the engine. Putting a large notebook across Linzmayer's knees, I took a pack of ESP cards out of my pocket and held it in my hand. He, meantime, had leaned back with his head resting against the top of the seat, so that his eyes saw nothing but the roof of the car. There were no mirrors or shiny surfaces into which he could have looked for possible reflections. During the actual progress of the test, his eyes were closed.

"After giving the pack a cut—neither of us knew the order of the cards in it anyway—I drew off the top one and tipped it toward me just enough to catch a glimpse of the symbol and then put it face down on the notebook on Linzmayer's lap. Without looking at it or touching it he said, after a pause of about two seconds:

'Circle.'

'Right,' I told him, drew off the next card, and laid it on the notebook.

'Plus,' he said.

'Right.'

'Waves.'

'Right.'

'Waves.'

'Right.'

At this point I shuffled the deck again, cut it once more, and again drew off a card.

'Star,' Linzmayer said when the card was placed on the notebook. It was a star.

When he had called *fifteen cards in succession* without a single mistake, both of us were too amazed for a while to go on with the rest of the run. No conceivable deviation from probability, no 'streak of luck' which either of us had ever heard of could parallel such a sequence of unbroken hits. We both knew that the thing Linzmayer had just done was virtually impossible by all the rules in the book of chance, but he had done it . . .

"No reader of this book need consider the account of this extraordinary run of Linzmayer's as presumptive evidence that ESP is a fact. The conditions of the test were not our usual laboratory ones, and the scientific evidence for ESP rests upon work performed under the strictest conditions. Write that amazing score off, if you like, to mere exploration. With all of the scepticism I can muster, though, I still do not see how any sensory cue could have revealed to Linzmayer the symbols of those 21 cards he called correctly" (pp. 77-79).

Confirmation of the present interpretation of the meaning of the experimental conditions prescribed by Rhine for ESP research is found in an article by Pratt and Price (79) on the subject-experimenter relationship. Blind Matching and Pack Matching tests were used and independent records were not made. "Favorable" and "unfavorable" conditions for high scores in ESP were defined as follows:

"The 'favorable' condition previously mentioned consisted of introducing a subject to the situation by one-half hour of general conversation before starting the tests and *then continuing the conversation during the test.*<sup>7</sup> The 'unfavorable' one was characterized by starting to test a subject without delay and in deliberately keeping him out of the conversation as much as possible" (p. 91).

The condition of divided attention in the experimenters may define ESP scoring ability in the subjects.

In closing this section on the conditions in subject and experimenter which foster ESP, some attention should be given to the general emphasis placed on "witnessing" by Rhine and those who have obtained evidence for ESP outside the Duke laboratory. It is an everyday observation that witnessing may, under some conditions, be absolutely untrustworthy. Witnesses, to serve any useful function, should take independent records of the calls and cards so that the accuracy of recording may be ascertained later.

<sup>7</sup> Italics mine.

Münsterberg (72) has discussed the well-known errors in human "witnessing" as they appear in courts of law. The general conclusion has been that "witnessing" is unreliable since a suggestible witness or one who expects high results may pay attention only to nonessential factors in the situation.

TABLE III

## THE "PSYCHOLOGICAL PHENOMENA" OF ESP

1. ESP usually exhibits an "insight" learning curve (83, p. 164).	Errors of attention and illusion are not products of gradual learning; hence their sudden intrusion as "insight."
2. Some agents are better than others (83, p. 101).	Some people are more prone to make these errors than others.
3. ESP fluctuates, waxes, and wanes (83, p. 137).	Errors in recording depend upon special conditions of expectancy, suggestibility, and inattention.
4. A carefree, playful attitude in both experimenter and subject is necessary for good ESP (83, p. 167).	These attitudes may also be best for unnoticed errors in recording.
5. Distances may increase extra-chance scoring (85).	When the percipient calls vocally in telepathy experiments, the chance for recording errors is increased; when the calls are later checked against the cards in clairvoyance experiments, errors may occur.
6. Drug results. High with caffeine, low with sodium amytal (83, p. 163).	Recorder knew what effect the drug ought to have. Caffeine may increase ability to use minimal cues; sodium amytal may decrease it.
7. Fatigue in the subject may decrease ESP scoring (83, p. 128).	Fatigue may lower subject's sensitivity to minimal cues.
8. ESP may be voluntarily controlled (75, pp. 204-205).	The direction of unnoticed errors may be controlled by recorder's preconceived hypothesis.
9. Certain psychoses may be differentiated by ESP tests (97, p. 104).	Same as 8.
10. Blind persons have ESP (80).	Same as 8.
11. The presence of sceptics disrupts good ESP scoring (83).	Sceptics may watch for sources of error and disrupt "playful" attitude.

From the above considerations of the "atmosphere" of the ESP laboratory, alternate explanations for the "psychological phenomena" of ESP research may be offered. Table III lists the phenomena and the present writer's conclusions as to their most probable explanation. It may well be that the absence of "playful informality" and consequent control of conditions in the experiments of Adams (1), Baker (3), Cox (24), Soal (103), and Willoughby (126) will help

to explain the lack of confirmation of the ESP hypothesis in these cases.

#### B. "INEXPLICABLE" EXPERIMENTS

Thus far, the criticisms and suggestions for improvement of experimental control brought forward apply only to the type of ESP experiments discussed above, where the conditions of the experiment, as stated in the papers themselves, are open to question. In this section, 2 experiments will be reviewed more completely since the reported conditions were such as to eliminate the errors discussed above. Eventual explanation of these results appears to the present writer to rest on an entirely different basis than the foregoing ESP data.

Warner (120) reported a set of 250 trials in which the conditions of the experiment were given as follows:

"... the experimenters, having told the subject that work was to begin, retired to the upstairsroom, shut the door, cut a shuffled deck of cards and placed the cut card face down on the table by itself. It was not seen by the experimenters until after the guess had been made and recorded by the subject in her room downstairs. The subject signalled when this had been done by pressing a button which flashed a light in the experimenter's room. When this signal was given, the card cut was exposed and recorded, and a different deck, newly shuffled, was cut to obtain the next card to be guessed. This procedure was repeated without interruption until the end of the work. At the conclusion, the subject's record of guesses was compared with the experimenter's record of cards cut" (pp. 236-237).

The average number of hits per 25 guesses was 9.30.

It is worth noting that recording was not completely independent in this experiment, since the signal could be varied in duration by the subject, thus providing a possible cue. Another unusual item about this report is the non-random distribution of frequencies of the card symbols: *i.e.* circle, 71; rectangle, 50; waves, 43; plus, 43; and star, 43. The chi-square, obtained by comparing theoretical and actual frequencies, is 11.76 with a *P* of .02 that repetition would give as bad or worse fit by chance. The call preferences, however, do not correspond with the most frequent card symbols. The possibility of variation in duration of the light signal serving as an unnoticed cue to the recorder should justify insistence on a repetition of the experiment under the same conditions with the card symbols recorded before the subject's guess and the uneven frequencies of the different symbols to be guessed eliminated. This technique involves

the added possibility of telepathy but this should not affect the scoring if the experiment is repeatable. The experiment was done in the subject's home.

Very high scores have been reported by Riess (92) with a single subject who later lost the ability to guess at an extra-chance level and who is not available for further experimentation. This experiment is unusual for the high scoring rate and the strictness of the conditions in ruling out the errors of ESP. For example, on the 19th run all 25 guesses were correct. The hits per pack were, consecutively, 5, 7, 10, 12, 15, 8, 16, 13, 18, 21, 11, 15, 19, 24, 21, 21, 22, 24, 25, 24, 21, 20, 19, 18, 14, 15, 15, 16, 12, 19, 21, 22, 24, 20, 18, 22, 21, 19, 19, 18, 18, 19, 18, 17, 18, 19, 20, 20, 20, 19, 20, 21, and 21. A period of no work intervened, after which the scores were 2, 4, 7, 12, 7, 5, 4, 3, 5, and 4. Obviously, the scores in the first series eliminate chance variation as an important factor in the production of the results. The experimental method is given as follows:

"The usual pack of ESP cards was employed. Since both the subject and the experimenter lived in the same suburb of New York City, the experiment was conducted in their respective homes. At 9:00 p.m. the experimenter would expose a card from the freshly shuffled deck lying on his desk. He looked at the card and noted the symbol on the mimeographed record sheet. At 9:01 p.m. the subject, seated at a table in her home, guessed at the symbol on the first card exposed by the experimenter in his home. The experimenter, meanwhile, had placed the first card to one side and at 9:01 had exposed the second card. This procedure was repeated until the deck was exhausted. It was then reshuffled manually and, after an interval of 10 minutes, the whole experiment was done again. In this way 50 trials were run off during each experimental session. The distance between the two homes was approximately one-quarter mile and the respective rooms in the houses were so situated that they faced in opposite directions to each other" (p. 261).

The recording was done independently, although apparently the subject was not witnessed. No information as to when and how the check-up for number of hits was made is included in the experimental report. Since the high scores reported in this experiment are so much at variance with the majority of ESP results, a full determination of the conditions under which the unusual results occur would seem to be of paramount importance.

In addition, the "Campus Distance Series in Clairvoyance," or the Pierce-Pratt series, should receive further mention, although this set of trials has been discussed already in the section on recording



errors. If a fuller account of the method is given, demonstrating that the check-up was made with completely independent records of cards and calls, this experiment should be joined with those of Warner and Riess as being as yet inexplicable.

#### IV. STATISTICAL METHODS IN ESP RESEARCH

Two major problems have arisen from controversy over ESP statistical methods. The first problem has to do with the mathematics of theoretical chance expectancy; the second, with the effect of selection upon the statistical methods used. The first problem has been fully discussed by Willoughby (123, 124, 125), Kellogg (55, 56, 57), Rogosin (93), Heinlein and Heinlein (46), Herr (47), Gulliksen (40), Wolfe (127), Stuart (106, 107, 108), Greenwood and Stuart (38), Stuart and Greenwood (110), Huntington (49, 50), Sterne (104), Lemmon (63), Greville (39), and Greenwood (35, 37). Kellogg (58) states that essential agreement has been reached on this theoretical point. To quote:

"... it is the mathematical aspect of the research upon which substantial agreement has now been attained. It is quite clear that the problems of selection and treatment of evidence as such, and of rigorous control of the experimental procedures, are approaching solution, but are not yet fully settled" (p. 148).

It is generally recognized, however, that the mathematics of chance expectancy does not indicate a functional relationship between variables. "Causes" for observed deviations from chance expectancy must be sought in the experimental conditions and controls, not in the mathematics of chance. This inductive "leap" from extra-chance scores to extra-sensory perception has been one of the underlying reasons for controversy.

Historically, one of the important considerations for evaluating guessing experiments in telepathy and clairvoyance has been the question of selection of data favorable to a preconceived hypothesis. No one will challenge the statement that extra-chance results can be obtained by selecting only deviations in the direction of the preconceived belief and putting them through a statistical mill which is not applicable to the scores because of the original selection. The selection-of-data error may be more subtle in its application in experimental situations. Three recent attempts to reproduce conditions thought to obtain in ESP experiments with respect to selection of data are available for analysis.

Warner (119) matched shuffled decks of ESP cards in order to obtain information about the "run of luck" explanation of extra-chance deviations. He selected 220 scores, from a large random distribution of hits per run, which were 8 or above. He then continued matching decks with respect to each of these scores until the total score (preliminary score plus further scores) fell below plus 1 sigma from the mean of 5 hits. Warner successfully demonstrated, then, that by chance alone, initial high scores tend to level out when enough random runs are added to the initial high score. Leuba (65), with the same general method, has demonstrated empirically that, as one would expect from theoretical considerations, significant deviations may occur on a purely chance basis.

Greenwood (37) recently suggested the use of a correction factor for the limit of chance expectancy, to be applied when the stopping point in a given series is a function of the results obtained up to that point.

## V. CONCLUSIONS

In the present review, the hypothesis was presented that extra-chance results in the majority of ESP experiments may be explicable on the basis of unnoticed errors in methodology. The errors and methods in which these errors may possibly occur may be summarized as follows:

1. Selection of data. All the ESP methods are open to this criticism in one form or another.
2. Lack of independent record. This criticism also applies generally to the ESP methods, with the exception, of course, of the experiments described above as "inexplicable."
3. Sensory cues. Results obtained with the Open Matching, Blind Matching, and Before Touching methods may be explained on this basis.
4. "Mental" habits. The Pure Telepathy methods are open to this criticism.
5. Logical inference. The BT methods in which the subject is given knowledge of results during a single run ( $BT_1$  and  $BT_s$ ) are unsatisfactory for this reason.

Certainly, the sources of error discussed here should be eliminated from consideration by adequate experimental control. These controls are not complex nor do they involve any novel principles. Greenwood (82) has already presented the main points of these suggestions.

The controls suggested for the ESP methods follow:

1. Sufficient distance or shielding should be used to eliminate all possibility of sensory cues.
2. Records of calls and cards should be made independently by 2 different recorders. The check-up should be carried out by juxtaposing the 2 independent records.
3. A tested method for producing a random distribution of symbols in the card series should be used.
4. In order to eliminate the effect of "optional stopping," a limit of trials should be set before the experiment. It might even be advantageous to postpone the check-ups until after the experiment is terminated.

Concerning the "inexplicable" experiments, little can be said. Perhaps another rule to insure objectivity should be added to the foregoing, namely:

5. The experiments should be conducted under such conditions and auspices that the subject cannot be suspected of fraudulently producing the results.

This rule would probably involve independent testing of the same "good" subject in different laboratories.

#### BIBLIOGRAPHY

1. ADAMS, E. T. A summary of some negative experiments. *J. Parapsychol.*, 1938, 2, 232-236.
2. ANONYMOUS SCIENTIST. Letter to the editors. *J. Parapsychol.*, 1938, 2, 65-70.
3. BAKER, K. H. Report of a minor investigation of extra-sensory perception. *J. exp. Psychol.*, 1937, 21, 120-125.
4. BARRETT, W. F., & BESTERMAN, T. The divining-rod; an experimental and psychological investigation. London: Methuen, 1926.
5. BARRETT, W. F., GURNEY, E., & MYERS, F. W. H. First report on thought reading. *Proc. Soc. psych. Res., Lond.*, 1882, 1, 13-34.
6. BAUCH, M. Psychologische Untersuchungen über Beobachtungsfehler. *Fortschr. Psychol.*, 1913, 1, 169-226.
7. BENDER, H. The case of Ilga K.: report of a phenomenon of unusual perception. *J. Parapsychol.*, 1938, 2, 5-22.
8. BESTERMAN, T. An experiment in long-distance telepathy. *J. Soc. psych. Res., Lond.*, 1932, 27, 235-236.
9. BIRD, J. M. Probabilities and metapsychics. *J. Amer. Soc. psych. Res.*, 1928, 22, 69-82.
10. BIRD, J. M. Experimental telepathy I. *J. Amer. Soc. psych. Res.*, 1929, 23, 517-533.
11. BIRD, J. M. Experimental telepathy II. *J. Amer. Soc. psych. Res.*, 1929, 23, 672-681.

12. BLACKBURN, D. *Lond. Daily News*, Sept. 1, 1911. [Not seen; taken from Coover (22).]
13. BOND, E. M. General extra-sensory perception with a group of fourth and fifth grade retarded children. *J. Parapsychol.*, 1937, 1, 114-122.
14. BRUGMANS, H. J. F. W. A communication on the telepathic experiments at the laboratory of psychology at Groningen. *C. R. premier Congr. int. Rech. psych.*, 1922, 396-408. [Not seen; quoted by Murphy (73, 74).]
15. BRUGMANS, H. J. F. W. L'état passif—d'un télépathe contrôlé par le phénomène psychogalvanique. L'état actuel des recherches psychiques d'après les travaux du II<sup>me</sup> Congrès International tenu à Varsovie en 1923. Paris: Presses Universitaires de France, 1924. Pp. 95-125.
16. BUTTON, W. H. The Margery mediumship. A few startling examples of thought-transference. *J. Amer. Soc. psych. Res.*, 1938, 32, 1-4.
17. BUTTON, W. H. The Margery mediumship. More about thought-transference. *J. Amer. Soc. psych. Res.*, 1938, 32, 40-47.
18. BUTTON, W. H. The mediumship of Mrs. Litzelmann. More about thought-transference. *J. Amer. Soc. psych. Res.*, 1938, 32, 65-68.
19. CARPENTER, C. R., & PHALEN, H. R. An experiment in card guessing. *J. Parapsychol.*, 1937, 1, 31-43.
20. CASON, H. A simple test for thought-transference. *J. Soc. psych. Res.*, 1924, 21, 314-319.
21. CHEVREUL, M. E. De la baguette divinatoire, du pendule, dit explorateur et des tables tournantes. Paris: Mallet-Bachelier, 1854.
22. COOVER, J. E. Experiments in psychical research at Stanford University. Stanford University: Stanford Univ. Press, 1917.
23. COOVER, J. E. Reply to critics of the Stanford experiments on thought-transference. (In press.)
24. COX, W. S. An experiment on extra-sensory perception. *J. exp. Psychol.*, 1936, 19, 429-437.
25. CROSLAND, H. R. An investigation of proof-reader's illusions. *Univ. Orc. Publ.*, 1924, 2. Pp. 168.
26. DRAKE, R. M. An unusual case of extra-sensory perception. *J. Parapsychol.*, 1938, 2, 184-198.
27. EDGEWORTH, F. Y. The calculus of probabilities applied to psychical research. *Proc. Soc. psych. Res., Lond.*, 1885, 3, 190-199.
28. EDGEWORTH, F. Y. The calculus of probabilities applied to psychical research II. *Proc. Soc. psych. Res., Lond.*, 1886, 4, 189-208.
29. Editorial comment. *J. Parapsychol.*, 1938, 2, 77-83.
30. ESTABROOKS, G. H. A contribution to experimental telepathy. *Boston Soc. psych. Res.*, 1927. Bull. V.
31. FARADAY, M. Experimental investigation of table-moving. *Athenaeum*, July, 1853, 801-803.
32. FERNBERGER, S. W. "Extra-sensory perceptions" or instructions? *J. exp. Psychol.*, 1938, 22, 602-607.
33. GIBSON, E. P. A study of comparative performance in several ESP procedures. *J. Parapsychol.*, 1937, 1, 264-275.
34. GOODFELLOW, L. D. The effect of patterns on psychophysical judgments. *Psychol. Bull.*, 1938, 35, 627. (Abstract of paper presented at the Columbus meetings of the A.P.A., 1938.)

35. GREENWOOD, J. A. Variance of the ESP call series. *J. Parapsychol.*, 1938, 2, 60-64.
36. GREENWOOD, J. A. Analysis of a large chance control series of ESP data. *J. Parapsychol.*, 1938, 2, 138-146.
37. GREENWOOD, J. A. An empirical investigation of some sampling problems. *J. Parapsychol.*, 1938, 2, 222-230.
38. GREENWOOD, J. A., & STUART, C. E. Mathematical techniques used in ESP research. *J. Parapsychol.*, 1937, 1, 206-225.
39. GREVILLE, T. N. E. Exact probabilities for the matching hypothesis. *J. Parapsychol.*, 1938, 2, 55-59.
40. GULLIKSEN, H. O. Extra-sensory perception—What is it? *Amer. J. Sociol.*, 1938, 43, 623-631.
41. GURNEY, E. Note relating to some of the published experiments in thought-transference. *Proc. Soc. psych. Res., Lond.*, 1889, 5, 269-270.
42. GURNEY, E., MYERS, F. W. H., & BARRETT, W. F. Second report on thought-transference. *Proc. Soc. psych. Res., Lond.*, 1882, 1, 70-97.
43. GURNEY, E., MYERS, F. W. H., PODMORE, F., & BARRETT, W. F. Third report on thought-transference. *Proc. Soc. psych. Res., Lond.*, 1882, 1, 161-215.
44. HALL, G. S. Experiments in telepathy. *Amer. J. Psychol.*, 1888, 1, 128-146.
45. HANSEN, F. C. C., & LEHMANN, A. Ueber Unwillkürliches Flüstern. *Phil. Stud.*, 1895, 11, 471-530.
46. HEINLEIN, C. P., & HEINLEIN, J. H. Critique of the premises and statistical methodology of parapsychology. *J. Psychol.*, 1938, 5, 135-148.
47. HERR, D. L. A mathematical analysis of the experiments in extra-sensory perception. *J. exp. Psychol.*, 1938, 22, 491-496.
48. HUMPHREY, B. M., & CLARK, J. A. A comparison of clairvoyant and chance matching. *J. Parapsychol.*, 1938, 2, 31-37.
49. HUNTINGTON, E. V. Exact probabilities in certain card-matching problems. *Science*, 1937, 86, 499-500.
50. HUNTINGTON, E. V. Is it chance or ESP? *Amer. Schol.*, 1938, 7, 201-210.
51. JASTROW, J. Chevreul as psychologist. *Sci. Mon.*, N. Y., 1937, 44, 487-496.
52. JEPHSON, I. Evidence for clairvoyance in card guessing. A report of some recent experiments. *Proc. Soc. psych. Res., Lond.*, 1929, 38, 223-271.
53. JEPHSON, I. A behaviorist experiment in clairvoyance. *Proc. Soc. psych. Res., Lond.*, 1932, 41, 99-114.
54. JEPHSON, I., SOAL, S. G., & BESTERMAN, T. Report of a series of experiments in clairvoyance conducted at a distance under approximately fraud-proof conditions. *Proc. Soc. psych. Res., Lond.*, 1930, 39, 375-414.
55. KELLOGG, C. E. Dr. J. B. Rhine and extra-sensory perception. *J. abnorm. (soc.) Psychol.*, 1936, 31, 190-193.
56. KELLOGG, C. E. New evidence (?) for extra-sensory perception. *Sci. Mon.*, N. Y., 1937, 45, 331-341.
57. KELLOGG, C. E. The problems of matching and sampling in the study of extra-sensory perception. *J. abnorm. (soc.) Psychol.*, 1937, 32, 462-479.



58. KELLOGG, C. E. Comments by Dr. Kellogg. *J. Parapsychol.*, 1938, 2, 147-148.
59. KENNEDY, J. L. The visual cues from the backs of the ESP cards. *J. Psychol.*, 1938, 6, 149-153.
60. KENNEDY, J. L. Experiments on "unconscious whispering." *Psychol. Bull.*, 1938, 35, 526.
61. KOLLARITS, J. Beobachtungen über Dyspraxien (Fehlhandlungen). *Arch. ges. Psychol.*, 1937, 99, 305-399.
62. KUBIS, J. F., & ROUKE, F. L. An experimental investigation of telepathic phenomena in twins. *J. Parapsychol.*, 1937, 1, 163-171.
63. LEMMON, V. W. Extra-sensory perception. *J. Psychol.*, 1937, 4, 227-238.
64. Letters and notes: ESP card imperfections. *J. Parapsychol.*, 1938, 2, 72.
55. LEUBA, C. An experiment to test the role of chance in ESP research. *J. Parapsychol.*, 1938, 2, 217-221.
66. MACFARLAND, J. D. Discrimination shown between experimenters by subjects. *J. Parapsychol.*, 1938, 2, 160-170.
67. MARTIN, D. R. Chance and extra-chance results in card matching. *J. Parapsychol.*, 1937, 1, 185-190.
68. MARTIN, D. R., & STRIBIC, F. P. Studies in extra-sensory perception: I. An analysis of 25,000 trials. *J. Parapsychol.*, 1938, 2, 23-30.
69. MINOT, C. S. The number habit. *Proc. Amer. Soc. psych. Res.*, 1886, 1, 86-95.
70. MINOT, C. S. Second report on experimental psychology—upon the diagram-tests. *Proc. Amer. Soc. psych. Res.*, 1889, 1, 302-317.
71. MÜNSTERBERG, H. Psychology and social sanity. New York: Doubleday, 1914.
72. MÜNSTERBERG, H. On the witness stand. New York: Clark-Boardman, 1927.
73. MURPHY, G. Telepathy as an experimental problem. In Murchison (Ed.), *The Case for and Against Psychical Belief*. Spencer, Mass.: Heffernan Press, 1927. Chap. 12.
74. MURPHY, G. Dr. Rhine and the mind's eye. *Amer. Schol.*, 1938, 7, 189-200.
75. PEGRAM, M. H. Some psychological relations of extra-sensory perception. *J. Parapsychol.*, 1937, 1, 191-205.
76. PFUNGST, O. Clever Hans (The horse of Mr. von Osten); a contribution to experimental, animal and human psychology. (Trans. by Carl L. Rahn.) New York: Holt, 1911.
77. PICKERING, W. H. Experiments on thought-transference. *Proc. Amer. Soc. psych. Res.*, 1886, 1, 113-116.
78. PRATT, J. G. Clairvoyant blind matching. *J. Parapsychol.*, 1937, 1, 10-17.
79. PRATT, J. G., & PRICE, M. M. The experimenter-subject relationship in tests for ESP. *J. Parapsychol.*, 1938, 2, 84-94.
80. PRICE, M. M., & PEGRAM, M. H. Extra-sensory perception among the blind. *J. Parapsychol.*, 1937, 1, 143-155.
81. PRINCE, W. F. The Sinclair experiments demonstrating telepathy. *Boston Soc. psych. Res.*, 1932. Bull. 16.
82. Research notes. *J. Parapsychol.*, 1938, 2, 72-73.

83. RHINE, J. B. Extra-sensory perception. Boston: Boston Society for Psychical Research, 1934.
84. RHINE, J. B. Some selected experiments in extra-sensory perception. *J. abnorm. (soc.) Psychol.*, 1936, 31, 216-228.
85. RHINE, J. B. The effect of distance in ESP tests. *J. Parapsychol.*, 1937, 1, 172-184.
86. RHINE, J. B. The question of sensory cues and the evidence. *J. Parapsychol.*, 1937, 1, 276-291.
87. RHINE, J. B. New frontiers of the mind. New York: Farrar & Rinehart, 1937.
88. RHINE, J. B. Experiments bearing on the precognition hypothesis. I. Pre-shuffling card calling. *J. Parapsychol.*, 1938, 2, 38-54.
89. RHINE, J. B., SMITH, B. M., & WOODRUFF, J. L. Experiments bearing on the precognition hypothesis: II. The role of ESP in the shuffling of cards. *J. Parapsychol.*, 1938, 2, 119-131.
90. RHINE, L. E. Some stimulus variations in extra-sensory perception with child subjects. *J. Parapsychol.*, 1937, 1, 102-113.
91. RICHET, C. La suggestion mentale et le calcul des probabilités. *Rev. Phil., Paris*, 1884, 18, 609-674.
92. RIESS, B. F. A case of high scores in card guessing at a distance. *J. Parapsychol.*, 1937, 1, 260-263.
93. ROGOSIN, H. Probability theory and extra-sensory perception. *J. Psychol.*, 1938, 5, 265-270.
94. SANGER, C. P. Analysis of Mrs. Verall's card experiments. *Proc. Soc. psych. Res., Lond.*, 1895, 11, 193-197.
95. SCHILLER, F. C. S. Review of "Experiments in Psychical Research" by J. E. Coover. *Proc. Soc. psych. Res., Lond.*, 1918, 30, 261-273.
96. SHARP, V., & CLARK, C. C. Group tests for extra-sensory perception. *J. Parapsychol.*, 1937, 1, 123-142.
97. SHULMAN, R. A study of card-guessing in psychotic subjects. *J. Parapsychol.*, 1938, 2, 95-106.
98. SIDGWICK, H. Involuntary whispering considered in relation to experiments in thought-transference. *Proc. Soc. psych. Res., Lond.*, 1897, 12, 298-315.
99. SIDGWICK, H., SIDGWICK, MRS. H., & SMITH, G. A. Experiments in thought-transference. *Proc. Soc. psych. Res., Lond.*, 1889, 6, 128-170.
100. SINCLAIR, U. Mental radio. New York: Boni, 1930.
101. SOAL, S. G. Methods in experimental telepathy. *J. Soc. psych. Res.*, 1931, 27, 130-135.
102. SOAL, S. G. Experiments in supernormal perception at a distance. *Proc. Soc. psych. Res., Lond.*, 1931, 40, 165-362.
103. SOAL, S. G. A repetition of Dr. J. B. Rhine's work in extra-sensory perception. *J. Soc. psych. Res.*, 1937, 30, 55-58.
104. STERNE, T. E. The solution of a problem in probability. *Science*, 1937, 86, 500-501.
105. STRATTON, G. M. The control of another person by obscure signs. *Psychol. Rev.*, 1921, 28, 301-314.
106. STUART, C. E. The Willoughby test of clairvoyant perception. *J. appl. Psychol.*, 1935, 19, 551-554.

107. STUART, C. E. In reply to the Willoughby "Critique." *J. abnorm. (soc.) Psychol.*, 1935, 30, 384-388.
108. STUART, C. E. A reply to Willoughby. *Character & Pers.*, 1935, 4, 80.
109. STUART, C. E. The effect of rate of movement in card matching tests of extra-sensory perception. *J. Parapsychol.*, 1938, 2, 171-183.
110. STUART, C. E., & GREENWOOD, J. A. A review of criticism of the mathematical evaluation of ESP data. *J. Parapsychol.*, 1937, 1, 295-304.
111. STUART, C. E., & PRATT, J. G. A handbook for testing extra-sensory perception. New York: Farrar & Rinehart, 1937.
112. SUDRE, R. Clairvoyance and the theory of probabilities. *J. Amer. Soc. psych. Res.*, 1928, 22, 63-69.
113. SUGDEN, E. H. Note on muscle-reading. *Proc. Soc. psych. Res., Lond.*, 1882, 1, 291-293.
114. THOULESS, R. H. Dr. Rhine's recent experiments on telepathy and clairvoyance and a reconsideration of J. E. Coover's conclusions on telepathy. *Proc. Soc. psych. Res., Lond.*, 1935, 43, 24-37.
115. TROLAND, L. T. A technique for the study of telepathy and other clairvoyant processes. Cambridge, Mass.: Author, 1917.
116. VAN LOON, F. H., & WEINBERG, A. A. A method of investigation into thought-transference. *J. Soc. psych. Res.*, 1921, 20, 4-23; 34-49.
117. VERRALL, MRS. A. W. Some experiments on the supernormal acquisition of knowledge. *Proc. Soc. psych. Res., Lond.*, 1895, 11, 174-197.
118. WARCOLLIER, R. Methods in experimental telepathy. *J. Soc. psych. Res.*, 1932, 27, 205-207.
119. WARNER, L. The role of luck in ESP data. *J. Parapsychol.*, 1937, 1, 84-92.
120. WARNER, L. A test case. *J. Parapsychol.*, 1937, 1, 234-238.
121. WARNER, L., & RAIBLE, M. Telepathy in the psychophysical laboratory. *J. Parapsychol.*, 1937, 1, 44-51.
122. WARNER, L., & RAIBLE, M. Summary of tests with miscellaneous subjects. *J. Parapsychol.*, 1938, 2, 149-150.
123. WILLOUGHBY, R. R. Prerequisites for a clairvoyance hypothesis. *J. appl. Psychol.*, 1935, 19, 543-550.
124. WILLOUGHBY, R. R. The use of probable error in evaluating clairvoyance. *Character & Pers.*, 1935, 4, 79-80.
125. WILLOUGHBY, R. R. A critique of Rhine's "Extra-Sensory Perception." *J. abnorm. (soc.) Psychol.*, 1936, 30, 199-207.
126. WILLOUGHBY, R. R. Further card guessing experiments. *J. gen. Psychol.*, 1937, 17, 3-13.
127. WOLFLE, D. L. A review of the work on extra-sensory perception. *Amer. J. Psychiat.*, 1938, 94, 943-955.
128. WOODRUFF, J. L., & GEORGE, R. W. Experiments in extra-sensory perception. *J. Parapsychol.*, 1937, 1, 18-30.
129. WOOLLEY, V. The broadcasting experiment in mass-telepathy. *Proc. Soc. psych. Res., Lond.*, 1928, 38, 1-9.

## STUDIES OF MENTAL RESEMBLANCE BETWEEN HUSBANDS AND WIVES AND BETWEEN FRIENDS

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The question whether opposites or likes attract in friendship and marriage has held enough popular interest for opposing views to become crystallized in two contradictory proverbs. Attempts to answer it in quantitative terms with respect to traits of intellect and personality have in recent years received a strong impetus from the development of devices for measuring such traits. The present review will consider, first, the material on mental resemblances of husbands and wives, then the literature on friend resemblance, and will conclude with a comparative and critical summary.

### STUDIES OF HUSBANDS AND WIVES

Reviews of the earlier literature on homogamy or assortative mating appear in studies by Jones (23), Schiller (35), and Schooley (36). This earlier material was quantitative in the field of physical characteristics, but largely subjective and speculative with regard to mental traits, particularly in the matter of personality resemblances. The field of temperament was the only one in which negative correlations were found.

The present article begins with the publications of 1928. In a number of cases the data concerning husbands and wives were incidental to a larger study of family resemblance, from which the material relevant to the present problem has been abstracted.

Table I summarizes the investigations of mental resemblance, classified, according to the type of characteristic studied, as intellectual, temperamental, or attitudinal. A compilation of results from a number of studies such as we have here leads one to look for agreements and disagreements. The similarity of the correlations in intellectual traits where the groups have been representative ones with a fairly wide range is worthy of note. Schiller's lower correlations may probably be ascribed to the homogeneity of her group of subjects. If the variability of the group has such a bearing on the significance of the correlation, one becomes conscious of the need for some indi-

TABLE I  
SUMMARY OF STUDIES OF MENTAL RESEMBLANCE OF HUSBANDS AND WIVES

Characteristic	Investigator	N of Couples	$r^1$	Remarks
<i>Intellectual</i>				
Intelligence	Willoughby (46, 47)	90	.44 <sup>2</sup> (av. corr.) .44 (" " " " )	A representative group from the vicinity of Palo Alto, California.
5 Verbal Tests	Jones (22)	105	.598 <sup>2</sup> ± .042	Group was representative of the general Vermont population in socioeconomic status.
6 Nonverbal Tests	Burks (5)	Group A 174 Group B 100	.42 ± .04 .55 ± .05	Groups chiefly from skilled labor, business, and professional classes.
Intelligence (Army Alpha)	Freeman, et al. (12)	150	.49 ± .04	Groups chiefly from skilled labor, business, and professional classes.
Intelligence (Stanford Rev.)	Schooley (36)	80	.556 ± .052	Range of school attendance for the men was 5 to 22 years.
Intelligence (Otis S-A)	Schooley (36)	80	.567 ± .051	
Intelligence (Otis S-A)	Schiller <sup>3</sup> (35)	46	.293 ± .090 (raw) .344 ± .088 (corr.)	Random pairs gave $r$ of —.138 ± .097 (corr.).
Memory (List of 20 words)	Schiller (35)	46	.109 ± .097 (raw) .117 ± .098 (corr.)	Random $r$ (corr.) was .086 ± .198.
Arith. Reasoning	Schiller (35)	46	.414 ± .082 (raw)	Random $r$ (raw) was —.077 ± .099.
Vocabulary	Schooley (36)	80	.468 ± .079 (raw)	Raw $r$ for random pairs:
Information	Schiller (35)	46	.378 ± .087	— .390 ± .086
Associations (Kent-Rosanoff)			.222 ± .097	.158 ± .099
Associations (Kent-Rosanoff)			.468 ± .079	.041 ± .102
Egocentric			.041 ± .102	— .461 ± .080
Supraordinate			.464 ± .080	— .121 ± .100
Contrast				
Inner				
Speech-Habit				

<sup>1</sup> Correlation coefficients are by the Pearson formula or its equivalent unless otherwise designated. It has not always been clear whether the coefficients were raw or corrected for attenuation. If correction is not mentioned, probably they may be assumed to be raw.  
<sup>2</sup> Coefficients are calculated from sigma scores for each age group to eliminate influence of age.  
<sup>3</sup> Schiller's subjects were a highly homogeneous group.



TABLE I—Continued

Characteristic	Investigator	N of Couples	$r^1$	Remarks
<i>Temperamental</i>				
Neurotic Tendency (Bernreuter)	Hoffeditz (17)	100	.160±.066	
Neurotic Tendency (Bernreuter)	Ternan & Buttenwieser (41)	126	.108±.060	A group scoring high in marital happiness.
		215	.217±.044	A group scoring low in happiness.
Neurotic Tendency (Bernreuter)	Sward & Friedman (39)	56	(raw) (corr.) .34±.08 .43	Jewish.
		56	.12±.09 .15	Gentile.
Neurotic Tendency (Bernreuter)	Crook (9)	79	.06±.070	
Neurotic Tendency (Thurstone)	Schooley (36)	80	.299±.069	
Neurotic Tendency (Thurstone)	Willoughby (48)	100?	.27±.05	Professional class.
Mental Hygiene Inventory (Woodworth-House)	Schiller (35)	46	.021±.098	Random $r$ was .059±.098.
Extroversion-Introversion (Whitman's Shortened Scale)	Schiller (35)	46	.005±.098	Random $r$ was .223±.093.
Introversion (Bernreuter)	Ternan & Buttenwieser (41)	126	.020±.060	High happiness score.
		215	.165±.045	Low happiness score.
Self-Sufficiency (Bernreuter)	Hoffeditz (17)	100	.091±.067	
Self-Sufficiency (Bernreuter)	Ternan & Buttenwieser (41)	126	.074±.060	High happiness score.
		215	.121±.045	Low happiness score.
Self-Sufficiency (Bernreuter)	Crook (9)	79	.00±.070	
Dominance (Bernreuter)	Hoffeditz (17)	100	.152±.066	
Dominance (Bernreuter)	Ternan & Buttenwieser (41)	126	.241±.057	High happiness score.
		215	.289±.042	Low happiness score.
Dominance (Bernreuter)	Crook (9)	79	-.05±.070	
Dominance (Allport A-S)	Crook & Thomas (10)	66	-.07	
<i>Attitudinal</i>				
Attitude Toward 17 Ideals	Hunt (19)	62		Median correlation in rank assigned to these ideals: For married couples, .475 For 62 random pairs, .255

TABLE I—Continued



cation of the variability of the groups with respect to neurotic tendency, dominance, or attitude toward Communism, for example. The series of comparisons to which one is tempted by the array of correlation coefficients is of uncertain justifiability. The correlations for the Allport-Vernon Study of Values are particularly open to question, since the several value scores for a given individual purport to be relative, not absolute, measures of the weights which the several values carry for that individual, and having a high score in one value automatically lowers the individual's rating in the sum of the remaining values.

With all these qualifications in mind, we may make certain generalizations concerning the results. The correlations are higher in the intellectual and attitudinal traits than in traits of temperament. None of the correlations, even in temperament, are reliably negative, as the theory of attraction of opposites would require.

An extensive survey of the problem which is not included in Table I because it is still in progress has been reported in a preliminary way by Kelly (24). This study of 300 engaged couples, to be followed up annually for 7 years, includes measures on the Otis Self-Administering, Bernreuter, Bell, Strong, Allport-Vernon, and several attitude tests, as well as anthropometric measures, blood groupings, and life history data. A preliminary analysis for the first 100 couples shows correlations ranging from approximately 0 to .79, none of them being significantly negative.

Some of the investigations have considered the question whether resemblances in personality are more pronounced for couples that have been longer married. Hunt (19) found a correlation of  $-.05$  between length of time married and similarity in ranking of 17 groups of ideals. Hoffeditz (17) compared 24 fathers and mothers aged 56 or more and 19 pairs of parents aged 45 or less with regard to resemblance in neurotic tendency, self-sufficiency, and dominance. She found no evidence that resemblance increases with duration of marriage. All the correlations were lower for the older couples, being slightly negative. The difference was least reliable for self-sufficiency, but approached reliability in the trait of neuroticism. Schooley (36), drawing the dividing line on the basis of length of marriage, and apparently at a considerably younger age in general, classified her 80 couples into a group of 40 who had been married from 1 to 4 years, and another group of 40 who had been married from 5 to 20 years. She found higher correlations for the longer-

married group in neurotic tendency, in the free association test, in the economic and religious values, and in attitude toward birth control; lower correlations for the longer married in the theoretical and aesthetic values; and no difference with length of marriage in correlation of scores on political value or on attitude toward Communism. Her disagreement with Hoffeditz regarding neurotic tendency may be due to the different basis of division or to the small number of cases. Newcomb and Svehla (29) partly agree and partly disagree with Schooley's results in so far as their data permit comparison. The trend which they find in attitude toward the church is similar to that reported by Schooley with respect to the religious value, but in attitude toward Communism they report a lower correlation for "fathers and mothers" than for "young husbands and wives." In attitude toward war they find a lower correlation for the longer married. The reliability of these differences in correlation is not great, however, and the data do not permit conclusion as to whether the longer mutual association has caused a change in degree of resemblance or whether the traits under consideration have at different periods held different degrees of importance in determining mutual attraction before marriage.

The relation of personality resemblance to marital compatibility has been investigated by Terman and Bottenwieser (40, 41) and by Kirkpatrick (25). Terman and Bottenwieser compared the degree of resemblance between 126 happily married couples, 215 couples with a low "combined happiness score" on a marriage questionnaire, and 100 divorced couples (the latter are omitted from our Table I). Among the correlations on the Strong and Bernreuter scales, the only one which was significantly higher for the happily married than for the other 2 groups was in the Y. M. C. A. interest constellation. On certain individual items there was a distinct difference between the happily and unhappily married with regard to the amount of resemblance between the pair. Husband and wife were more likely to be happy if they were similar in attitude toward avoiding argument, and more likely to be unhappy if they resembled each other in admitted ability to "accept just criticism without being sore." Out of 130 items in which there was more than a chance difference between the relative amount of correlation yielded by the first group and the other 2, there were 96 in which agreement accompanied happiness, 30 in which agreement was associated with unhappiness, and 4 in which the relationship was uncertain. Terman and

Buttenweiser (40) also found that between groups selected for high and low happiness score there were consistent differences in scores for "common outside interests" and in agreement on 11 items which included "religious matters" and "philosophy of life."

Kirkpatrick (25) likewise found significant differences between unhappily and happily married couples in a measure of community of interest in a variety of activities. He considers these differences significant enough so that the "Relationship Family Interests" and "Individual Family Interests" scores might serve as an index for measuring marital adjustment and for predicting the success of a relationship.

#### STUDIES OF FRIENDS

Study of the resemblance between friends is an aspect of social psychology which has received increasing attention in recent years. Evidence of the recency of this interest appears in the temporal distribution of the 21 pertinent titles in the accompanying bibliography: 12 bear dates from 1931 to 1937 inclusive, 7 from 1922 to 1930, and the other 2 were published in 1898 and 1902, respectively. These studies reflect to a considerable degree the techniques which were available at the time when they were made, and the special channels in which the interests of psychologists were then directed. The 2 studies at about the turn of the century by Street (38) and Bonser (4) obtained from each subject a direct statement concerning the resemblance between himself and a friend in "temperament" and in "likes and dispositions." The 3 studies in 1922 and 1923 by Almack (1), Warner (43), and Williams (45) were concerned with chronological age, mental age, and IQ. As various standardized measures of personality traits appeared, resemblance in these traits became the object of investigation, beginning with Wellman's (44) use of the Marston extroversion-introversion scale in 1926. Statistical analysis in most of the studies prior to 1927 is not carried as far as in the later ones. Prior to 1927, Almack (1) was the only one to use the correlation technique. Of the 7 studies previous to 1929, 6 were concerned with the elementary or high school age, and 1 with ages 17 to 21. Beginning with 1929, 6 of the studies have been concerned with college friendships, 5 with children and adolescents, and 3 with preschool children.<sup>4</sup>

<sup>4</sup> This figure includes merely the 3 studies which deal with other factors than chronological age and sex in relation to choice of companions.



*Preschool Friendships.* During the preschool age, social contacts between children increase with increasing years, according to the observations of Beaver (2), Moreno (27), Parten (30), Salusky (34), and Zaluzhni (50). Where the age range in the group is wide enough to permit considerable range of choice, it has been found that children tend to select companions of similar age (Chevaleva-Janovskaja, 8; Challman, 7; Green, 15). Where the age range is 18 months or less, as in the groups observed by Hagman (16) and Hubbard (18), chronological age does not appear to influence the selection of companions within the group. Cleavage on the basis of sex was found by Hagman (16) to be absent in a two-year-old group but present in a four-year-old group. Chevaleva-Janovskaja (8) likewise found that the tendency to form unisexual associations increased with age. Challman (7) reports a marked tendency for preschool friends to be of the same sex.

The 3 studies which have considered other factors than chronological age and sex in relation to choice of companions are those by Challman (7), Hagman (16), and Hubbard (18). Resemblance in mental age appeared to be related to companionship in Hubbard's group of 18 children aged 21 to 39 months, and among Hagman's 18 two-year-olds; and the same holds true for IQ among Hagman's two-year-olds. Resemblance in mental age and IQ and extroversion were found to be unrelated to paired companionship among Hagman's 24 four-year-olds and in Challman's group of 33 children ranging from 27 to 59 months in age. Among two-year-olds, Hagman obtained a correlation of  $-.476 \pm .134$  in extroversion when each child was paired with his most frequent companion. Similarity with regard to attractiveness of personality (adult ratings) was found by Challman to be unimportant, and Hagman found the "Social Stimulus Index" similarly unimportant among her two-year-olds, though of some significance among the four-year-olds. In both age groups the most important factor related to choice of companions found by Hagman was similarity in Social Reaction indices. Challman similarly reports that resemblance in social participation was the most important factor in friendships between girls, and that resemblance in sociality apparently carries some weight in determining friendships in either sex. Hubbard, however, found that similarity in social participation was unimportant in her group. This inconsistency may be due to a difference in methods of measurement or to the small number of cases in all the groups compared. Similarity in degree of

physical activity bears some relation to friendships in both sexes, according to Challman.

*Friendships at the Elementary and High School Level.* At the elementary and high school level resemblance between friends in intelligence, especially mental age, is reported by Almack (1), Warner (43), Williams (45), Furfey (13), Jenkins (20), Partridge (31), Seagoe (37), and Pintner, Forlano, and Freedman (33). Socioeconomic status of parents was found by Jenkins (20) to be of great importance. The influence of propinquity appears strongly in studies by Furfey (13), Seagoe (37), and Pellettieri (32). Resemblances rather than differences between friends were found in likes and dispositions (Bonser, 4), social maturity (Furfey, 13), play interests (Jenkins, 20), and a number of personal characteristics, notably athletic ability, courtesy, and cleanliness (Seagoe, 37). Data presented by Wellman (44) reveal less friend resemblance in extroversion than in scholarship (girls) or IQ (boys). Pintner, Forlano, and Freedman (33) found friend correlations near 0 on measures of cultural attitudes, ascendance, extroversion, and emotional stability.

*Friendships at the College Level.* In connection with a study in moral education, Street (38), in 1898, found that out of 189 persons (mostly girls between the ages of 17 and 21) who replied to his questionnaire, 46 reported that they were attracted by persons of opposite temperament, 43 by similar persons, 50 gave no clue, and 50 confused the issue.

Five more studies of college friendships are summarized in Table II. It is evident that similarity rather than dissimilarity is the rule, though the correlations are low or moderate. This general trend is in harmony with that of questionnaire replies received by Bogardus and Otto (3) from 138 men and 162 women students concerning interests, attitudes, and abilities.

*The Criterion for Friendship.* The studies of resemblance between friends present a problem which is not found in the studies of marriage partners: the matching of the subjects in pairs. The criteria for friendship have been of 2 main sorts: observed association, and the designation of one friend by the other. Each of these is open to a certain amount of criticism. Friendship may be inferred from the fact that 2 persons are frequently seen together, but it is not thereby proved. As for designation of one friend by another, the unsatisfactoriness of this criterion is indicated by the fact that A may name B as his "best friend," but B may name K and make no reference

TABLE II

SUMMARY OF STUDIES OF RESEMBLANCE OF COLLEGE FRIENDS

Characteristic	Investigator	N of Pairs	r	Remarks
Socioeconomic Status (Sims)	Flemming (11)	23 45	.20 .24	Men. Women.
Intelligence (Thorndike)	Garrett (14)	110	.21	Men.
"	Flemming (11)	21 45	.12 .51	Men. Women.
" (Army Alpha)	Flemming (11)	48 60	.14 .11	Men. Women.
" (Thurstone)	Vreeland & Corey (42)	15	-.30±.14	Both members of each pair were of same sex, but some pairs men, some women.
Social Intelligence (G. Washington Univ.)	Garrett (14)	110	.27	Men.
"	Flemming (11)	18 11	.44 .54	Men. Women.
"	Vreeland & Corey (42)	30	.40±.10	See note above.
Neurotic Tendency (Thurstone)	Vreeland & Corey (42)	30	.33±.11	" " "
Introversion-Extroversion (Laird)	Flemming (11)	32 59	.56 .13	Men. Women.
Pleasiness (Rating)	Flemming (11)	48	.34	Men.
Emotional Steadiness (Rating)	Flemming (11)	61 48 61	.53 .17 .20	Women. Men. Women.

TABLE II—Continued

Characteristic	Investigator	N of Pairs	r	Remarks
Emotional Expressiveness (Rating)	Flemming (11)	47	.15	Men.
		61	.03	Women.
Social Adjustment (Rating)	Flemming (11)	48	.38	Men.
		61	.42	Women.
Attitude Toward War (Droba)	Vreeland & Corey (42)	30	-.016 ± .12	See note above.
Public Opinion Test (Watson)	Vreeland & Corey (42)	30	.09 ± .12	" "
Opinions on Foreign Affairs, Race, Religion, Economic and Political Questions	Winslow (49)	86	.24 ± .07	Same-sexed pairs, both men and women.
Will-Character (Rating)	Cattell (6)		.35 ± .09	Men.
Surgency of Temperament	"		-.10 ± .09	Women.
Perseveration	"			62 students were asked each to name 1 intimate friend from the group, and 1 person disliked. Some named more than 1. Number of pairs is not stated. Differences in amount of resemblance between friends and enemies were small and of low reliability, but showed that similarity favored compatibility except in perseveration.
Fluency of Association	"			

to A.<sup>5</sup> The most satisfactory evidence would appear to be found where A and B name each other as best friends. Probably each criterion assures some degree of friendship, however, or indicates that in some way the pair are related more than by a random matching. Moreover, even under the same criterion not all the friendships will be equal in intimacy.

Table III classifies the studies according to the criterion for friendship which was adopted by the investigator, and also indicates the ages of the subjects. In only one study, that by Seagoe (37), were all the pairs mutually named and free from overlapping. The figures which we quote from this investigator show what a wide net was cast in order to obtain a group of 29 pairs who twice designated each other as first choice.

#### COMPARISONS AND CONCLUSIONS

In view of the facts that the present measures of personality traits still leave something to be desired, that the groups measured have differed widely in age and in degree of homogeneity, that the criteria for pairing of friends have not been entirely satisfactory, and that there were different degrees of compatibility among the husbands

<sup>5</sup> The question whether or not choices are mutual receives especial attention in the sociometric studies of Moreno (27). Pupils in an elementary public school and a private preparatory school were asked to indicate whom they would like to have sit near by, and girls in the New York State Training School were asked whom they would prefer for housemates. Persons were found who "like stars, capture most of the choices, others forming mutual pairs, sometimes linked into long mutual chains or into triangles, squares, or circles, and then an unlooked-for number of unchosen children." In one "social atom," A might be attracted to B, C, D, E, F, and G; B, C, and D might reject A; and G might be the only one positively attracted to A.

Moreno's approach differs from that in the other investigations included in the present review. The aim was to study the network of psychological currents in a group with regard to its bearing on the group adjustment. Individuals were interviewed as to the reasons for their attractions or repulsions. There is little statistical analysis of the factors which led to "clicking" or the reverse. Among the generalizations from some of the interviews reported in a supplementary section by Jennings (21) appear the statements that "children appear to choose associates according to attributes necessary for the joint pursuit of common aims" (third grade); "the motivations are often based on similarities of traits, physical and mental, of social standing, and of interests in common pursuits. . . . The rejections are . . . based largely on differences, physical and mental" (fifth grade); "occasionally choices are made motivated by complementary attributes" (seventh grade).



and wives, such consistencies as appear among the results of these studies are all the more worthy of note.

Throughout all the traits and the range of ages the correlations

TABLE III

CLASSIFICATION OF STUDIES OF FRIENDS ACCORDING TO THE CRITERION FOR  
FRIENDSHIP

A. *Observed Association.*

Systematic Recording of the Number of Times the Subjects Were Seen Together.

Challman (preschool)  
Hagman (preschool)  
Hubbard (preschool)  
Wellman (elementary school)

Self-selected Groups at Summer Camp.

Partridge (adolescent boys)

Close Companionship Recognized by Others.

Vreeland & Corey (college)  
Warner (elementary school age, boys' gangs)

B. *Designation of One Friend by Another.*

Without Regard for Mutuality.

Almack (elementary school)

First choice for inviting to a party.

First choice to help in work.

Bogardus & Otto (college): "chum."

Bonser (high school): "chum."

Cattell (college): "one intimate friend." (Some named more than one.)

Flemming (college): "best friend."

Out of 61 women there were 38 mutual designations.

Out of 48 men there were 18 mutual designations.

Garrett (college): "one best friend."

Pintner, Forlano, & Freedman (elementary school)

Each child listed his 3 best friends in order of preference. In 3 out of 4 school groups, the friends named had to be from the child's own grade.

Street (ages 17 to 21 years)

Williams (delinquent adolescents): 2 "chums" named by each.

Winslow (college)

Each student who answered the questionnaire also gave it to 1 friend.

Mutual Naming.

Furfey (Boy Scouts)

35 boys each named the ones he most liked to play with. 62 mutual pairs were found.

Partridge (adolescent boys)

Each boy named 3 camp chums. Only mutual pairs were included.

Seagoe (elementary school)

First choice for inviting to a party.

Out of 823 children, 115 choices were mutual. Group I.

Out of these 115 pairs, 29 chose each other again 1 month later.

Group II.

C. *No Statement Regarding Criterion.*

Jenkins (junior high school)

between the paired scores of friends or marriage partners have been positive with very few exceptions.

At all ages, with the possible exception of part or all of the preschool period, a tendency to resemblance in intelligence was found. Where the population sample under consideration showed a relatively narrow range—notably in the case of college friends and in Schiller's group of married couples—the correlations in intelligence were low; otherwise they were moderately high. This influence of the variability of the group upon the size of the correlation for mental age appears clearly in the study by Pintner, Forlano, and Freedman (33). When Grades 5 to 8 were included in the group, the correlations ranged from .45 to .62 in various schools and for varying friend combinations. When a single-grade group was under consideration, the correlations ranged from .05 to .17. These data suggest that among children a large part of the friend correlation in mental age is due to a related chronological age factor. In the adult studies which cover a wide social-economic range it would be desirable to know how much correlation in intelligence scores would remain if the social-economic factor were partialled out.

In traits of temperament, the correlations at most ages have tended to run considerably lower than in intelligence, though they are still positive in the great majority of cases. The question may be raised whether the low and unreliable coefficients are due to a lack of a definite trend with respect to resemblance, or whether they are partly a by-product of the unreliability of the available measuring instruments in this field. It is in the field of temperament that the one negative correlation which approached reliability appeared: the correlation for extroversion in a group of 15 two-year-olds.

In attitudinal traits appear the greatest differences between the results for the several age groups. At the elementary school level, Pintner, Forlano, and Freedman (33) found correlations in measures of cultural attitudes to be near 0. Between husbands and wives, however, correlations in attitude scores are among the highest that have been found in any type of trait. The fact that they are lower between college friends may be due to an age factor again; the data are meager, however. The field of attitudes and interests appears to be one of the most promising approaches to the study of marital compatibility, judging from Kirkpatrick's (25) success in discriminating between the happily and the unhappily married by means of

his measures of community of interests, and from some of Terman and Bottenwieser's results (40, 41). Further studies of this type are desirable.

## BIBLIOGRAPHY

- ✓ 1. ALMACK, J. C. The influence of intelligence on the selection of associates. *Sch. & Soc.*, 1922, 16, 529-530.
- ✓ 2. BEAVER, A. P. The initiation of social contacts by preschool children: a study of technique in recording social behavior. *Child Developm. Monogr.*, 1932, No. 7.
- ✓ 3. BOGARDUS, R., & OTTO, P. Social psychology of chums. *Sociol. soc. Res.*, 1936, 20, 260-270.
- ✓ 4. BONSER, F. G. Chums: a study in youthful friendships. *Ped. Sem.*, 1902, 9, 221-236.
5. BURKS, B. S. The relative influence of nature and nurture upon mental development. *27th Yearb. nat. Soc. Stud. Educ.*, 1928, Part I, 219-321.
6. CATTELL, R. B. Friends and enemies: a psychological study of character and temperament. *Character & Pers.*, 1934, 3, 54-63.
- ✓ 7. CHALLMAN, R. C. Factors influencing friendships among preschool children. *Child Developm.*, 1932, 3, 146-158.
- ✓ 8. CHEVALEVA-JANOVSKAJA, E. Les groupements spontanés d'enfants à l'âge préscolaire. *Arch. Psychol., Genève*, 1927, 20, 219-233.
- ✓ 9. CROOK, M. N. Intra-family relationships in personality test performance. *Psychol. Rec.*, 1937, 1, 479-502.
- ✓ 10. CROOK, M. N., & THOMAS, M. Family relationships in ascendance-submission. *Publ. Univ. Calif. Educ., Phil., Psychol.*, 1934, 1, 189-192.
- ✓ 11. FLEMMING, E. G. Best friends. *J. soc. Psychol.*, 1932, 8, 385-390.
- ✓ 12. FREEMAN, F. N., HOLZINGER, J., & MITCHELL, B. C. The influence of the environment on the intelligence, school achievement, and conduct of foster children. *27th Yearb. nat. Soc. Stud. Educ.*, 1928, Part I, 103-217.
- ✓ 13. FURFEY, P. H. Some factors influencing the selection of boys' chums. *J. appl. Psychol.*, 1927, 11, 47-51.
- ✓ 14. GARRETT, H. E. Jews and others: some group differences in personality, intelligence, and college achievement. *Person. J.*, 1929, 7, 341-348.
- ✓ 15. GREEN, E. H. Friendships and quarrels among preschool children. *Child Developm.*, 1933, 4, 237-252.
- ✓ 16. HAGMAN, E. P. The companionships of preschool children. *Univ. Ia Stud. Child Welf.*, 1933, 7, No. 4.
- ✓ 17. HOFFEDITZ, E. L. Family resemblances in personality traits. *J. soc. Psychol.*, 1934, 5, 214-227.
- ✓ 18. HUBBARD, R. M. A method of studying spontaneous group formation. *Child Developm. Monogr.*, 1929, No. 1, 76-85.
- ✓ 19. HUNT, A. M. A study of the relative value of certain ideals. *J. abnorm. (soc.) Psychol.*, 1935, 30, 222-228.
- ✓ 20. JENKINS, G. G. Factors involved in children's friendships. *J. educ. Psychol.*, 1931, 22, 440-448.

21. JENNINGS, H. Sociometric studies. In Moreno, J. L., *Who Shall Survive? A New Approach to the Problem of Human Interrelations. Nerv. ment. Dis. Monogr. Ser.*, 1934, No. 58, 373-425. ✓
22. JONES, H. E. A first study of parent-child resemblance. *27th Yearb. nat. Soc. Stud. Educ.*, 1928, Part I, 61-72.
23. JONES, H. E. Homogamy in intellectual abilities. *Amer. J. Sociol.*, 1929, 35, 369-382.
24. KELLY, E. L. A preliminary report on psychological factors in assortative mating. *Psychol. Bull.*, 1937, 34, 749. (Abstract.) ✓
25. KIRKPATRICK, C. Community of interest and the measurement of marriage adjustment. *Family*, 1937, 18, 133-137.
26. KIRKPATRICK, C., & STONE, S. Attitude measurement and the comparison of generations. *J. appl. Psychol.*, 1935, 19, 564-582.
27. MORENO, J. L. Who shall survive? A new approach to the problem of human interrelations. *Nerv. ment. Dis. Monogr. Ser.*, 1934, No. 58.
28. MORGAN, C. L., & REMMERS, H. H. Liberalism and conservatism of college students as affected by the depression. *Sch. & Soc.*, 1935, 41, 780-784.
29. NEWCOMB, T., & SVEHLA, G. Intra-family relationships in attitude. *Sociometry*, 1937, 1, 180-205.
- ✓ 30. PARTEN, M. B. Social participation among preschool children. *J. abnorm. (soc.) Psychol.*, 1932, 27, 243-269.
31. PARTRIDGE, E. D. A study of friendships among adolescent boys. *J. genet. Psychol.*, 1933, 43, 472-477.
32. PELLETTIERI, A. J. Friends: factors involved in friendship-making among adolescent boys. Nashville: Author, 1935.
33. PINTNER, R., FORLANO, G., & FREEDMAN, H. Personality and attitudinal similarity among classroom friends. *J. appl. Psychol.*, 1937, 21, 48-65.
- ✓ 34. SALUSKY, A. S. Collective behavior of children at a preschool age. *J. soc. Psychol.*, 1930, 1, 367-378.
- ✓ 35. SCHILLER, B. A quantitative analysis of marriage selection in a small group. *J. soc. Psychol.*, 1932, 3, 297-319.
- ✓ 36. SCHOOLEY, M. Personality resemblances among married couples. *J. abnorm. (soc.) Psychol.*, 1936, 31, 340-347.
- ✓ 37. SEAGOE, M. V. Factors influencing the selection of associates. *J. educ. Res.*, 1933, 27, 32-40. ✓
38. STREET, J. R. A study in moral education. *Ped. Sem.*, 1898, 5, 2-40.
39. SWARD, K., & FRIEDMAN, M. B. The family resemblance in temperament. *J. abnorm. (soc.) Psychol.*, 1935, 30, 256-261.
- ✓ 40. TERMAN, L. M., & BUTTENWIESER, P. Personality factors in marital compatibility: I. *J. soc. Psychol.*, 1935, 6, 143-171.
- ✓ 41. TERMAN, L. M., & BUTTENWIESER, P. Personality factors in marital compatibility: II. *J. soc. Psychol.*, 1935, 6, 267-289. +
- ✓ 42. VREELAND, F. M., & COREY, S. M. A study of college friendships. *J. abnorm. (soc.) Psychol.*, 1935, 30, 229-236. ✓
43. WARNER, M. L. Influence of mental level in the formation of boys' gangs. *J. appl. Psychol.*, 1923, 7, 224-236.

- ✓44. WELLMAN, B. The school child's choice of companions. *J. educ. Res.*, 1926, 14, 126-132.
- ✓45. WILLIAMS, P. E. Study of adolescent friendships. *Ped. Sem.*, 1923, 30, 342-346.
- ✗46. WILLOUGHBY, R. R. Family similarities in mental test abilities. *Genet. Psychol. Monogr.*, 1927, 2, 235-275.
- ✗47. WILLOUGHBY, R. R. Family similarities in mental test abilities. 27th *Yearb. nat. Soc. Stud. Educ.*, 1928, Part I, 55-59.
- ✗48. WILLOUGHBY, R. R. Neuroticism in marriage: IV. Homogamy. *J. soc. Psychol.*, 1936, 7, 19-31.
- ✓49. WINSLOW, C. N. A study of the extent of agreement between friends' opinions and their ability to estimate the opinions of each other. *J. soc. Psychol.*, 1937, 8, 433-441.
- ✓50. ZALUZHNI, A. S. Formation of social habits in children of preschool age. In Murphy, G., & Murphy, L. B., *Experimental Social Psychology*. New York: Harper, 1931. Pp. 282-283.

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## BOOK REVIEWS

SKINNER, B. F. *The behavior of organisms*. New York: Appleton-Century, 1938. Pp. ix+457.

Skinner proposes a system for the convenient formulation of behavioral data, and then proceeds to describe experiments which test the system. The book represents the culmination of a program of research originally projected in a semihistorical doctoral dissertation at Harvard on the concept of the reflex. The experimentation, concerned almost exclusively with the lever-pressing activity of rats, began to appear in 1930. The book summarizes previous reports and adds new data. In order to estimate the success with which the author achieved his purposes, tentative answers to three questions will be attempted: (1) What are the chief characteristics of the system which he proposes? (2) Of what significance are the experimental findings, both as a validation of his system, and in relation to the problems of psychology conceived in other ways? (3) Is the system as formulated and supported experimentally likely to become a competitor to other existing systems of psychology? These are difficult questions, which cannot be answered confidently. Yet to ask any less significant questions would be to underestimate the task which the author set himself.

1. *The Nature of the System*. Skinner proposes what is strictly a science of behavior, therefore neither a mental science nor a neural science. His system is in this respect in keeping with current trends as represented, for example, in the writings of Guthrie, Hull, Lewin, and Tolman. In none of these systems is there recourse to neuroanatomy. Skinner takes a firm stand in favor of descriptive positivism, against hypotheses. "A purely descriptive science is never popular. For the man whose curiosity about nature is not equal to his interest in the accuracy of his guesses, the hypothesis is the very life-blood of science" (p. 426). "Deductions and the testing of hypotheses are actually subordinate processes in a descriptive science, which proceeds largely or wholly without hypotheses to the quantitative determination of the properties of behavior and through *induction* to the establishment of laws" (p. 437). In this he is, of course, outside the trends currently popular in psychology.

Since the structure of a descriptive system is determined by its subject matter, it is pertinent to inquire how the subject matter is selected. It is evident that two influences have directed Skinner's choice of representative behavior. In the first place, he believes that the reflex is the analytical unit which makes possible the scientific investigation of behavior (p. 9). The reflex is not to be thought of in neural terms, however, but is to be defined as a correlation between stimulus and response. The choice of the reflex as the analytical unit determines the

general formulation of the 'laws' which include after-discharge, temporal summation, refractory phase, facilitation, inhibition, conditioning, extinction, and so on. It is evident that the laws are not discovered or formulated entirely *de novo*, but derive largely from Sherrington, Magnus, and Pavlov. They are all redefined operationally to apply to behavior without neurological implication, and as so defined they are not the laws of spinal reflexes. As stated, they do not appear to the reviewer to be laws at all, but collections of variables probably correlated in such ways that laws might be looked for. To describe them as laws of behavior is like speaking of a 'law of moisture' or a 'law of sunshine' as laws of growth at the stage when little more is known than that moisture and sunshine favor growth. The 'laws' do, however, direct the inquiry, and hence are surrogates for hypotheses. The choice of the rat's lever-pressing for food as the representative reflex was probably dictated by the desire to show that precise relationships much like those of the neurologists could be validated within behavior which physiologists would not be tempted to call reflex.

It would be a mistake to give the impression that Skinner makes a careless use of analogy in calling a rewarded act a reflex or in adopting the physiologist's names for the laws which describe this act. His is a formal and sophisticated system, and when he does violence to the contemporary socially accepted concept of the reflex, he knows very well what he is doing. Definitions are given with extreme care.

The real significance of the selection of a rewarded act as the representative behavior apparently became clearer to Skinner as the experiments progressed, for one of the more important distinctions did not appear in the published reports until 1937. This is the distinction between *respondent* behavior, which, like an ordinary reflex, is *elicited* by a precise stimulus, and *operant* behavior, which is not elicited by identifiable stimuli but may be said to be *emitted*. This is the behavior, sometimes called random or spontaneous, important in trial-and-error situations such as that which Skinner studies. Respondent behavior is significantly correlated with antecedent stimuli; the relations into which operant behavior enters are different. It is operant behavior which is strengthened when lever-pressing is rewarded. It does not matter what caused the rat to depress the lever the first time; once the operant response has produced food, the operant is strengthened. When operant behavior is correlated with a stimulus, the situation always involves discrimination. The discriminated stimulus is really only a cue or occasion for the behavior, not a true stimulus to elicit the behavior. The distinction between respondent and operant has been implicit in Thorndike's work all along, but it did not become explicit because the use of 'situation' to cover discriminated as well as eliciting stimuli permitted a spurious application of the stimulus-response formula. This clear distinction is perhaps Skinner's most significant conceptual contribution. He hopes to correct the disproportionate emphasis upon respondent behavior by basing all of his work on operant behavior. Having formulated laws after the pattern of reflex physiology, Skinner's problem is to validate and quantify the laws within operant behavior.

The respondent-operant distinction is an important one in setting up two types of conditioned reflex. Pavlov's variety is based on respondents, and because the correlation of response is with substituted stimuli, this is designated as Type S. Skinner's variety strengthens a response (an operant) by rewarding it, and to emphasize the response this is designated as Type R. Actually, Pavlov's experiments are not pure illustrations of Type S, but for expository purposes a fairly stereotyped description of Pavlov's experiment is used by Skinner. The distinction between these types is that made earlier by Thorndike between associative shifting and trial-and-error.

2. *The Experimental Data.* The bulk of the book is devoted to experimental findings in which the rate of response in the lever-pressing situation is correlated with many pertinent variables: drive, reinforcement, nonreinforcement, delayed reinforcement, periodic reinforcement, discriminatory stimuli, and differentiated response. The data are presented chiefly in the form of 148 figures, most of which are reproduced kymograph records. Many uniformities are demonstrated, confirming the position that lawfulness may be found in this situation. The result is lawfulness rather than new or reformulated laws. It is difficult to determine within a positivist system just what level of generality constitutes a law. The laws formally stated before the experimentation is reported are not resummarized after the data have been discussed. It is to be supposed that they were found adequate. If this interpretation is correct, the laws were merely definitions of variables to be investigated, and experimental verification means not that the laws are proved or disproved, but merely that the variables chosen were convenient to direct inquiry.

The real quantitative laws are not, then, the laws formally stated, but the equations which fit the reported curves in each specific instance. There is a uniformity about the eating rate under standard conditions which may be expressed by the law that  $N=kt^n$ , where  $N$  is the number of pellets eaten in time  $t$ , with  $k$  and  $n$  appropriate constants. This is never specifically called a law, but it is as near to one as any relationship which Skinner reports. There are many relationships of this kind which are important contributions both to the factual knowledge of behavior and to methodology in behavioral investigations. One or two illustrations may be added to indicate the richness of the data. After a single reinforcement (once receiving a pellet following lever-pressing), there follow a number of responses to the lever although pressing is no longer reinforced. This yields a characteristic extinction curve. Probably no other conditioning method provides as sensitive an indicator of the result of a single reinforcement. The concept of the 'reflex reserve' emerges, to be distinguished from momentary strength. The 'reflex reserve' is the potential number of responses to be made without further reinforcement: it might be called 'resistance to extinction,' in more conventional terminology, although this does not define it adequately. A further demonstration of considerable methodological interest is provided under the concept of 'periodic reconditioning.' When responses are reinforced every three minutes or every six or nine or twelve minutes, a character-

istic uniform rate of responding results, represented graphically by a straight line of slope varying with the interval. The number of responses under standard conditions is relatively constant, say eighteen per reinforcement. This value is characterized as the 'extinction-ratio.' Because of the linearity of the response curve within periodic reconditioning, it is feasible to use this curve to test the influence of other variables, such as differences in drive. Periodic reconditioning is not to be confused with reinforcement at a fixed ratio. That is, if every tenth response is reinforced, the result is not uniformity of response, but acceleration. Ratios as large as one reinforcement for every 192 responses are reported; under these circumstances very high rates of responding occur, showing positive acceleration between reinforcements similar to that which would be predicted from Hull's goal-gradient hypothesis. These few specimens can only suggest the great variety of relationships which have been explored, many of which are distinctly new and should be assimilated to the body of psychological knowledge.

3. *Estimate in Relation to Other Systems.* In choosing a representative sample of behavior, Skinner has been restricted by his bias in favor of the reflex. Having made the choice for operant behavior against respondent behavior he believes himself to have chosen more representative behavior than that usually chosen, i.e. by Sherrington and Pavlov (p. 438). Although he is outspoken in his denunciation of a science of behavior which subordinates itself to neural science, he is more conspicuously aware of neurologists and physiologists than of psychologists. Respondent behavior is, after all, not very prominent in Ebbinghaus, Freud, McDougall, the later Thorndike, Gestalt, and in many other behavioral systems less physiological than Skinner's. Had he chosen to modify their systems, rather than the systems of those working with reflexes, he might have developed an entirely different program, based on different representative behavior. It is interesting in this connection to note that he devotes a whole chapter to clarifying his service to neurology, with which he has broken, but he devotes only three pages specifically to the systems of other psychologists with which his work is coördinate. The statements about Lewin, Hull, and Tolman on these pages are intelligent, but cursory. Tolman's system is recognized as the nearest relative. Thorndike, another close relative, is ignored in this comparison.

It is unfortunate that Skinner did not do his readers the service of relating his system in greater detail to the experimental data of other investigators. His own comment is significant: "There is no implication whatever that this is the only important work that has been done in the field, but simply that I have had little luck in finding relevant material elsewhere because of differences in basic formulations and their effect on the choice of variables to be studied" (p. 47). If Skinner has been unable to relate his work to that of other investigators, how can a reader, coming fresh upon this new body of material, be asked to make the transitions? The difficulties in making the extensions of the system may result in the book's being less useful, and perhaps less influential, than it ought to be.

That Skinner's task of going beyond his own experiment would not have been insurmountable is evident through the studies now beginning to appear from Hull's laboratory, in which Skinner's situation is used, but

the results of which are reported in accordance with more familiar conventions. The expedient of adding a second lever coördinates his situation with choice-point behavior, so important in other systems. It may be that these and related studies will result in bringing to Skinner's work the attention which it deserves.

ERNEST R. HILGARD.

Stanford University.

OGDEN, R. M. *The psychology of art*. New York: Scribner, 1938. Pp. xviii+291.

In the early years of his academic life R. M. Ogden acquired an interest in things aesthetic at least partly through his contacts with W. A. Hammond and O. Külpe. In 1905 he published the *Esthetic attitude* and in 1907 he was the cotranslator with Max Meyer of A. Hildebrand's *The problem of form in painting and sculpture*. Since that time he has published an occasional treatise on the psychology of the arts, the best known being his book, *Hearing*, which appeared in 1924. The scope of these offerings has not been narrowly confined but has dealt with such widely different problems as psychical distance, consonance, naïve geometry applied to painting and architecture, and schools of art. The present book can be considered as touching on all of these interests.

The title, *The psychology of art*, adequately indicates the book's contents only if one accepts a rather restricted view of psychology. The treatment is observational and quantitative, but rarely experimental in the sense in which modern psychologists employ the term. Of the few experimental findings noted, the majority are to be found in the auditory sections. The only American psychologists to whom reference is made are G. T. Buswell, R. C. Givler, and H. S. Langfeld, and the German-Americans included are W. Köhler and K. Lewin. The contents reveal the interests of a well-trained psychologist who has a tremendous interest in and a large knowledge of the arts. In consideration of the treatment, a more adequate title might have been *The arts as a psychologist sees them*.

The author defines the aesthetic as "a felt behavior, the pattern of which lacks discernment." The artistic is "the perfection of means to ends, a perfection which becomes artful only as the means themselves become an end" (p. 16). As a Gestalt psychologist, Ogden warns against too static a type of aesthetic analysis. Birkhoff's well-known M, for example, is far too static for a place in the author's system. Every work of art is a figure-ground pattern which is nonenumerative.

After an introductory section, two chapters are devoted to the problems of music. The material is essentially, with a few extensions, what is to be found in Max Meyer's *The musician's arithmetic*. Ogden's treatment is rather uneven. Valuable space needed to amplify the description of difficult material is occasionally devoted to what is essentially grade-school material, e.g. over a page is devoted to a description of the several sorts of rests and notes. On the whole, however, the treatment is concise and fair.

Poetry is discussed under the major headings of literary art and



prosody. The subheadings are: poetry and prose, the nature of literary art, linguistic sound, rhyme, assonance, alliteration, rhythm, literary style, the poetic foot, poetic metre, modern poetry, poetic inspiration, and literary forms. The material, in the main, is far-removed from the psychological laboratory. Ogden's treatment, therefore, is forced to be largely that of the professor of English. The chapters, nevertheless, make interesting reading.

The five chapters devoted to visual art are profusely illustrated with photographs and figures. The more important of the currently accepted art principles are described and many psychological findings are shown to bear directly on the problems of the arts. Attention is paid to the tectonic arts as well as to drawing, painting, and sculpture. The author dislikes the currently popular "modern house." To justify this dislike he has presented a photograph at which even the enthusiasts for modern housing will shudder. His choice of a photograph to illustrate the modern factory building (which, incidentally, he likes) is a much happier one.

Throughout these chapters the author's 'gestaltish' inclination is shown by frequent references to figure and ground, by the mention of "strong figures," by a lack of interest in preferences for isolated colors and forms, and by the quest for functional entities. The discussion of the last-mentioned quest furnishes what is perhaps the most intriguing portion of the book. Art objects are analyzed in an attempt to learn whether they show static or dynamic symmetries. Naïve geometries are assumed to have operated in the construction of the famous art objects of antiquity. In speaking of architecture Ogden says: "It is surely not a matter of chance that measurements of the whole permit an analysis of its members into subtle geometrical proportions" (p. 239).

Many aestheticians and psychologists will not hold with Ogden that geometry has these intimate connections with the arts. They may see, for example, little relation between our high regard for the Parthenon and the fact that its Euthynteria contains two squares and two root-five rectangles. Yet it is the reviewer's guess that they cannot but be fascinated by Ogden's discourse on golden sectors, root rectangles, Pythagorean stars, whirling squares, and all the other geometric oddities.

The last section is devoted to an appeal for eurhythmics, for "the use of right rhythms in postural behavior" (p. 272). As Ogden conceives the issue, our pedagogy should retrieve, if possible, the rhythmic procedures of classical Greece. Dalcroze's more narrowly conceived system of eurhythmics furnishes a step in the correct direction. To this should be added the rhythmic approach to vocalization and verbalization. "Aesthetic pleasure and efficient performance go hand in hand" (p. 272).

The Carnegie Corporation of New York should be thanked for financing Ogden's *The psychology of art*. Except for the section on music, the Ogden book overlaps very little the other modern volumes devoted to the psychological aspects of aesthetics. It is packed with interesting observations and speculations. For these reasons it should find its place on the book shelf of every psychologist who has an interest in some one of the arts.

PAUL R. FARNSWORTH.

Stanford University.

FREDERICK, R. W. *How to study handbook*. New York: Appleton-Century, 1938. Pp. xxviii+442.

So many crimes have been committed in the name of secondary school handbooks on "How to Study" that one has come to look askance at any volume bearing this title. Because volumes of this sort have issued chiefly from the hands of well-intentioned but uninformed folk, the appearance of a carefully written handbook which bears the earmarks of firsthand acquaintance with cognate psychological researches is an event worth noting. Frederick's manual unquestionably belongs in the latter category. Instead of presenting speculative 'rules' proclaimed as true by a man of experience, Frederick has produced a series of discussions which are uniformly aligned with the literature of experimental psychology. Furthermore, he has, without loss of dignity, written on a level suitable for secondary school students. Some portion of the credit should go to W. H. Burton for the editorial assistance acknowledged by the author, but the coherence of the final product leaves no doubt as to the soundness of the groundwork itself.

The volume opens with a dozen brief chapters on reading, which occupy a total of ninety-eight pages. The exposition is clear and thought-provoking. One might wish for a more extensive treatment of remedial measures, but one cannot quarrel with the correctness of the discussion that is presented. Indeed, there are few college students who would fail to profit from a careful perusal of this section.

Later chapters deal with such topics as listening in class, gathering materials for reports, making notes, and dealing with examinations. The chapter on experimentation seems to this reviewer to present too simple a view of the case, and the chapters on thinking might with profit have been carried beyond the conventional limits. The author does occasionally slip into careless use of such psychologically flavored terms as 'unconscious' and 'subconscious' and he does go into details which have nothing to do with studying and which only serve to interrupt the march of the exposition. These shortcomings, however, are not of great importance, and they are more than nullified by the empirical tenor and logical coherence of the volume. It may be enough to say that many psychologists will probably be pleased to see it in the hands of the teachers of their own offspring.

JOHN G. JENKINS.

*University of Maryland.*

REID, A. C. *Elements of psychology: an introduction*. New York: Prentice-Hall, 1938. Pp. xix+409.

The appearance of a new textbook in general psychology is hardly a novelty these days. Hall's concern in the period of the nineties about the "multiplication of textbooks" would appear to be more germane if expressed today than in Hall's own day. It is not easy to classify current textbooks in general psychology. Undoubtedly, however, it would be fair to state that many texts show the influence of functional and dynamic concepts—perhaps the dominant psychological mode in America. Several current texts utilize behavioristic concepts and Gestalt interpretations.

A great many—perhaps a large majority—are undeniably eclectic in treatment. There appears to be a slight tendency to write psychology texts down to the level of the great mediocrity. This trend is hazardous for psychology or any other science. (The above statement is not to imply, however, that there may not be a field for books written in a humanistic vein.)

Taking as a point of reference the current modes of psychological treatment, Reid's psychology appears to be anachronistic; for "Elements of Psychology" is a thoroughgoing introspectionism. The writer has undoubtedly been influenced very greatly by the teachings and writings of the late Professor Titchener, and the pattern of the book resembles closely the Titchenerian model. Since mind is defined in terms of conscious content, it will be obvious that the text omits a discussion of many topics that are of current interest in psychology, such as perceptual-motor learning, motivation, intelligence, and personality.

Of the 379 pages of text, 130 pages, or slightly over a third of the book, are devoted to the topic of sensation, including intensity relations. Other orthodox topics, from the introspective point of view, are, of course, image, affection (sensation, image, and affection are regarded as mental elements), perception, association and memory, attention, emotion, and action.

The material of the book is systematically organized. The style is clear and concise, and the treatment is serious and dignified. For any teacher who might wish to introduce psychology from an introspective point of view, the reviewer would recommend the book without equivocation. So far as the reviewer was able to detect, the book is relatively free from errors. However, he was disturbed by the definition of 'retroactive inhibition': "Retroactive inhibition is the condition in which the process of forming associations seems to stop some time prior to the moment at which one's experiences cease." The discussion following this statement seems to indicate that the author has in mind what is generally referred to as anterograde amnesia. From an etymological standpoint, this might be a possible usage of the term; however it has come to have a technical meaning in psychology which is quite different from this, and which has widespread acceptance.

PAUL L. WHITELY.

*Franklin and Marshall College.*

POWERS, F. F., MCCONNELL, T. R., TROW, W. C., MOORE, B. V., & SKINNER, C. E. Psychology in everyday living. Boston: Heath, 1938. Pp. x+511.

The purpose of the authors in writing this volume was "to meet the need of the college student for a well-integrated, modern text which will materially contribute to his ability to manage his own life, and to the realization of certain other fundamental purposes of a general liberal education." This book is intended for those "students who, for the most part, will never become professional psychologists." Hence, "such concepts as managing one's own life, personality development, planning a

career, socialization, learning, and other practical applications have been stressed." In the words of the authors "the volume is not just another traditional textbook."

An attempt is made not only to present "the facts of empirical psychology, but also to show their implications where possible." Therefore, to this end, the book, consisting of twenty-one chapters, is divided into five parts: (1) "The Nature of Psychology," (2) "Understanding Ourselves," (3) "Adjustment to College and Life," (4) "Learning and Study," and (5) "Applications of Psychology."

Chapters I and II deal with topics that are usually considered in the introductory chapters of texts written for beginners. These chapters, however, are flavored somewhat with practical suggestions, and, the writer thinks, should prove to be rather stimulating to the beginner. Many of the traditional topics usually treated in a text intended for beginners are omitted. Others are briefly treated in Chapters III to IX, inclusive. The discussions of the determinants of behavior, the physiological and functional principles, and types of behavior are inadequate. The treatment of receptors, connectors, and effectors is hardly more than an outline of these parts. Some valuable and practical suggestions are offered in the chapters dealing with personality and adjustments. Although the treatments of emotions, intelligence, motivation, and individual differences are simple in nature, they are modern in substance and thought-provoking.

There is a tendency on the part of the authors to do a lot of listing and enumerating. Throughout the entire book discussions are brief. Hence, the writer thinks that perhaps beginning students who use the book as a text will be apt to learn *more about* psychology and *less of* psychology. However, this approach to a study of the science may be the better one. If and when a student learns a lot about a science, his interests usually become more profound in it, and he is stimulated to further study in that field. Often, the first course taken in a field of study furnishes the basis for a decision, on the part of the student, either to take more courses in that field or to drop the study there. In that case, the comprehension of the real nature and subject matter of psychology might well be left for succeeding courses.

The materials discussed in Part Three, comprising Chapters X, XI, and XII, rightly belong to the fields of social psychology, vocational guidance, and matrimonial guidance. The brief consideration of problems in these fields might well be extended. While the treatment is brief, many timely suggestions are offered that should prove to be most valuable.

The discussion of the learning process, constituting all of Part Four, continues for five chapters. This consideration, including a discussion of the formation of study habits, is the most exhaustive treatment in the book. The authors avoid all controversial topics and theories in this field. Also, they do not advocate any new theories and short cuts in the learning process.

Part Five is a treatise on applied psychology. The fields of application here considered are business, industry, law, politics, religion, medi-

cine, and education. It is surprising to one to learn how much practical psychology is to be found in the volume. Relatively few footnotes are employed, but copious references are to be found in connection with the various chapters. In addition, there is an appendix containing a long list of supplementary books.

OSBORNE WILLIAMS.

*University of Florida.*

STRANG, R. An introduction to child study. (Rev. ed.) New York: Macmillan, 1938. Pp. xv+681.

"It is hoped that this book will prove useful to individual parents . . . ; to parents' clubs, county demonstration groups, and other organizations of adults; to parent-teacher associations; to advanced high-school students in home-economics classes; to teachers . . . ; to students in liberal arts colleges . . . ; and to students of psychology . . . ; as well as to students in teachers' colleges, normal schools, and nurses' training schools, for whom the book is primarily intended." Dr. Strang's statement of the composite reader-group for whom she is writing explains to the critical reader the essentially elementary character of the book. She has succeeded in keeping the style and treatment simple enough for the comprehension of high school students and of so-called "average" parents. By so doing she has undoubtedly oversimplified to the point that the work has lost some of its value as a textbook for the more advanced groups for which she also designed it. This is inevitable; and it is probable that the greater need today is for a book which will meet the needs of the larger group of less specialized students, elementary teachers, nurses, and parents.

In her selection of material the author has been no less restricted than in delimiting her audience. Obviously, it is conceivable that, in its broader scope, "child study" does include material from a great variety of approaches, including those of medicine, pediatrics, anthropology, and nutrition, as well as those of psychology, education, and "child training." However, it is a Herculean task to attempt to unify material from so great a variety of sources, and it is not surprising that some sections of the book are little more than compendia of useful information.

In developing her material, Dr. Strang has followed a strict chronological order, from the prenatal period through adolescence. Each section is divided into chapters on development, how the child learns, special problems of the period, and suggestions on how to study the child. There is evidence of thorough familiarity with the literature of the subject, although on occasion the tendency to simplify leads the author to draw somewhat more definite conclusions than those of the original study. Each chapter is followed by a list of "questions and problems for class discussion or study groups"; each section carries an annotated bibliography of standard books in the field. From time to time there are blanks in which a mother may record the development of her own child, and compare this with the norm. The appendix contains height-weight tables for children from one month to twelve years of age, and a summary of



play materials for children from birth to eighteen years of age. (This latter was prepared by Dr. Katherine B. Greene.)

In spite of the various individual excellences of the book, it is unfortunate that the general impression on the reader is that of a handbook of helpful hints, which might have been compiled from daily articles in a newspaper column. For the lay mother it probably has a certain value which might justify giving it a place on the shelf between the cookbook and the *World Almanac*.

HELEN MARSHALL.

*Stanford University.*

JUNG, C. G. *Psychology and religion*. New Haven: Yale Univ. Press, 1938. Pp. 131.

Dr. Jung works from the facts which his clinical experience discovers to broad and far-reaching hypotheses which, in turn, largely control his clinical methods—an entirely proper practice which is used by all scientists. Dr. Jung, no more and no less than others, faces the danger that facts which support his basic thesis are more easily discerned than those which apparently oppose it. Since the most fundamental hypothesis which Dr. Jung accepts is distinctly different from those accepted by the majority of psychiatrists, psychologists, and students of religion generally, his Terry Lectures for 1938 are somewhat difficult to read appreciatively.

The basic thesis which Dr. Jung accepts is that "self" is the totality of conscious and unconscious existence, a summation of individual and racial experience of which the latter is the more important both in bulk and in dynamic quality. Every individual experience contains something unknown, since some of its content comes from a psyche more complete than consciousness. In other words, it is a mistake to consider the individual human psyche as a merely personal affair.

The unconscious portion of the self makes itself obvious only in special circumstances. In emergencies, for instance, unexpected, new, and strange instinctive forces appear. Character is amazingly changed. In neurosis much the same kind of thing occurs. There is a manifestation of a power and a meaning which is not yet understood but which has a devastating effect upon even highly rationalistic and intellectual men.

The modern mind has forgotten those old truths which speak of the death of the "old man" and the making of the new, of spiritual rebirth, and so on. The modern attitude is to look back upon the mists of credulity and superstition from which man has emerged with pride, forgetting that he carries all of the past in his unconscious. The reviewer at this point is uncomfortably uncertain whether or not Dr. Jung regards the disappearance of superstition and credulity as regrettable. But perhaps he means that since, in his view, these have to exist somewhere it is better that they exist on the conscious level.

Religious experience seems to have to do with the unconscious areas of self which are, now and then, faced consciously. Such confrontation is terrifying and from it man seeks refuge in dogma and ritual. That is to say, religion is a substitute which replaces the immediate experience

of unconsciously retained racial history with symbols which are vested in creed and ritual authoritatively supported by an institutional church or by evangelistic fervor. Dogma represents the self more completely than any scientific theory since the latter expresses the conscious mind alone. Ritual, largely abandoned by Protestants, to their partial undoing, has always been a safe and pleasant way of dealing with the unaccountable forces of the unconscious mind.

Religion is a relationship to the highest or strongest value. That psychological fact which is the greatest power in any system is its Deity. Any religion rooted in the history of a people is a true exposition of its psychology; that is, of its fears, desires, and frustrations. This relationship is expressed in symbols, the likeness of which in different religious groups proves the existence of an archetypal image of Deity—not at all the same thing as proving the existence of Deity, but very important nevertheless.

There is more in this compact little volume than this. Dr. Jung is convinced that dreams, especially those with any repeated content, are revelatory of the unconscious territories of mind. Dreams, he says, are visible links in a chain of unconscious events. In his argument he works from dream analysis to the more general theories reported above. There is also an interesting description of the quaternity symbol and of what the author calls the "heretical attempt to improve on the dogma of the Trinity."

*Psychology and religion* is interesting and valuable after the reader has mastered a somewhat difficult style. When properly considered as phenomena of social psychology rather than as a *sacrosanctus* beyond the possibility of intelligent consideration, religion must be recognized as of enormous importance. Every attempt at fundamental analysis is welcome. Dr. Jung has made a contribution of importance to scholars, though one more easily understood by devotees of his variety of psychoanalysis than by those who are skeptical of the whole psychoanalytic approach. The book is not intended for and has little to offer one who may be termed 'the practical religious worker.'

GEORGE R. WELLS.

*Hartford Seminary Foundation.*

PARTRIDGE, E. D. *Social psychology of adolescence*. New York: Prentice-Hall, 1938. Pp. xv+361.

In writing the *Social psychology of adolescence*, Partridge set for himself the task of surveying the fields of psychology and sociology as they relate to young people. He emphasizes the rôle of patterns of culture as determinants of behavior of the young individual; hence, adolescence is attributed importance more because of social implications attendant upon development than because of biological processes occurring at that age. The point of view adopted is that an understanding of the behavior of young people entails a study of the "total configuration of the individual as a part of a large and complicated social scheme."

Chapters I and II are introductory to the main treatment of adolescence. In them Partridge stresses again and again the *relationships* in

the environment as they influence behavior. A field-theoretical approach, Gestalt method, is suggested as a means of attacking these relationships. The timeworn arguments in favor of Gestalt methods and the typical Gestaltian attacks on analytical psychology are given. This controversial matter might well have been omitted. It can contribute little to a lay reader, or even an elementary student in psychology; furthermore, the treatment of subsequent portions of the book is little influenced by it.

In the remaining chapters of the book, the social psychology of adolescence is presented. As one would anticipate, the physical and physiological facts pertaining to adolescence are not stressed. Recognition is given them, however, as influences upon the personal and social adjustment of the individual. The author discusses the nature of the individual with respect to his adaptability through learning and through the adoption of common ways of maintaining integrity: retreating, rationalizing, sublimating, etc. The social implications of individual differences in mental abilities and physical characteristics are then presented. Following this, the informal group is characterized and its influences on individual behavior are given. Adolescent leaders and their influences on individuals are ably discussed in the light of numerous investigations. The remainder of the book is a comprehensive treatment of the influences on behavior of the group, the sexes, the family, leisure time, the community, education, and factors which lead to delinquency and other abnormalities.

For use as a one-semester text on adolescence this book has many virtues and few defects. It is interesting to read and is readily comprehensible. While it contains few figures, graphs, and tables, it has adequate contemporary references given in footnotes and at the end of each chapter. The transition from chapter to chapter is orderly and coherent. The author does not deviate from the field of study, the process of acquiring social maturity. This coherence is due in part to his contention that few characteristics of development are peculiar to adolescence. Thus he recognizes that development is continuous and that processes beginning early in life affect behavior as a constant rather than as a periodic influence. The book should be more teachable than its contemporaries which treat separately and often unrelatedly the development of such aspects as physique, intelligence, emotion, motivation, morality, and personality.

JOHN B. WOLFE.

*University of Mississippi.*

GREGG, F. M. *The psychology of a growing personality.* Lincoln, Nebraska: Personality Press, 1938. Pp. xv+489.

In *The psychology of a growing personality*, Gregg presents a McDougallian-tinged view of psychology for character educators and nonacademic men and women. He discusses the general topics of introductory psychology, personality, and mental hygiene around an outline of "stages" of individual development: Babyhood, Dramatic Age, Big-Injun Age, Age of Loyalty, Mate-seeking Age, Romantic Age, Adulthood.

Much of the material of the book came from lectures which the author had given to parents, teachers, and lay audiences. Hence, it is not surprising that little experimental data are given and, further, that the bibli-

ography is heavily weighted with popular and secondary source materials. Stories and anecdotes are used extensively as interest-holding devices and as vehicles for character education, as the author conceives it.

Readers of this book may gain improper impressions from it. They are told, for example, that conditioning from environmental influences begins at the moment of birth; that an IQ of 77 means that the individual possessing it is 77% as smart as the average boy or girl of his chronological age; that educational psychologists are environmentalists partly because they have had little background in neurology; that extreme behaviorism (present-day behaviorism is ignored) is undesirable and frequently leads an individual into sensuality; that there are seven psychological theories of why people behave, four theories of personality determination, ten schools of psychology. Too much stress is placed upon instinctive tendencies as explanations of behavior and upon precarious "stages" into which development of behavior has been classified.

JOHN B. WOLFE.

*University of Mississippi.*

BLUEMEL, C. S. *The troubled mind: a study of nervous and mental illnesses.* Baltimore: Williams & Wilkins, 1938. Pp. ix+520.

A common procedure for books dealing with psychopathology is to give rather scanty descriptive material accompanied by extensive theoretical interpretations. Bluemel reverses this emphasis and presents a wealth of excerpts from the behavior records of patients while devoting relatively little space to the exposition of views as to why they behave as they do. Furthermore, instead of dwelling largely upon cases of extreme mental derangement, he devotes about 70% of his book to the milder forms of psychoneuroses.

He takes up in order some fifty symptoms. His general procedure is to give a description of the symptom and then to illustrate from case records the several ways in which this symptom may appear. He does not clutter up his case descriptions with irrelevant items the way so many writers in this field do.

The manifold forms that psychopathic behavior may take are made exceedingly clear by the multiplicity of types which Bluemel describes. To those persons who have not had extensive contact with psychotic patients this book should be exceptionally illuminating. It should help to dispel the current lay notion that there is some common factor running through all mentally disturbed individuals.

There is no doubt that the author himself considers the greatest contribution of his book to be the application of the principles set forth to an understanding of psychopathic individuals who are at large and especially those in public life. He considers the aggressive psychopath who manages to gain some official position to be more dangerous than the more modest psychotic individual who keeps his maladjustments a private matter. The drive of the public-spirited psychopath may lead to social unrest, political upheaval, war, and revolution.

The book is not intended for psychiatrists or specialists but for those individuals who must deal with people and who might be helped in their social contacts by substituting understanding for intolerance. Because

of this aim the book says very little about treatment and the methods of treatment that are suggested are usually of a very general nature. Bluemel gives some theoretical discussion of the way in which the principles of learning and conditioning may be invoked to explain functional mental abnormalities but does not fall into the error of attempting to trace all abnormalities to some such central factors as the libido, inferiority complex, Oedipus complex, instincts, or the like.

The reader who is not familiar with psychopathology may be rather confused after reading this book because of the great variety of behavior types which are discussed, but this confusion is probably more wholesome than the false assurance that comes to the reader of an oversimplified treatment of mental diseases. The facts in this field are complicated and Bluemel does not gloss over differences in any attempt to ride a hobby.

Although the greatest emphasis is given to the description of symptoms, the author does not leave the reader with the impression that the removal of symptoms is the essential aim of therapy. He shows very clearly that these symptoms are the result of more deep-seated difficulties and, while temporary measures are sometimes demanded, sound procedure involves a search for the more basic causal factors.

The person who knows nothing of psychopathology can read this book with interest and profit, and the expert in the field will find the book very refreshing.

JOHN J. B. MORGAN.

*Northwestern University.*

ADAMS, R. *Interracial marriage in Hawaii*. New York: Macmillan, 1937. Pp. xvii+353.

Dr. Adams' book on racial intermarriage has for a factual foundation census reports, supplemented further by annual reports of the Governor of Hawaii. The discussion of the various population trends, as shown by these figures, is based, however, on Dr. Adams' own observations and experience as professor of sociology in the University of Hawaii, and leads directly to an interesting interpretation of the customs and attitudes of the various ethnic groups resident there. It is primarily a sociological document, but contains considerable material of value to social psychologists.

The general outlines of the history of immigration to Hawaii are widely known, but not the details. The early coming of the first Polynesian navigators who crossed the immense open spaces of the Pacific, first to discover and then to populate the islands, is lost to view except through the shadowy media of myth and tradition. The whites were the next discoverers and the earliest nonnative settlers. Then came in successive waves the peoples who now make up the bulk of Hawaii's population: Chinese, Portuguese, Japanese, Koreans, Puerto Ricans, Filipinos—each group contributing its share to the racial composite.

Hawaii has been called many things: the Paradise of the Pacific, a racial laboratory, the crossroads of the Pacific, America's listening post towards the Orient—all designations more or less deserved.

From the human standpoint, an interracial poker game in which the whites have assumed the perpetual right to shuffle and deal the cards



and call the turn would be, in the reviewer's opinion, as apt an analogy as any. That it is as friendly a game, with the players borrowing and lending among themselves—and occasionally taking a peek at their opponents' hands—is rather extraordinary. To those who are interested in these adjustments and an authoritative account of when the players arrived and what they paid to sit in the game, Adams' book will be a useful guide. Racial intermarriage was and is the outstanding feature of these group adjustments and determines more than anything else who can borrow from whom.

Space will not permit any extended review of the author's observations and interpretations of the acculturation processes in Hawaii, but as the Japanese are the most numerous group and represent the player about whose hand the dealer is most uncertain, we might pay some attention to the chapter which deals with their social organization.

The Japanese, in 1930, constituted about 38% of the total population and four years later this proportion had not shrunk. Meanwhile, the fact that 44% of the female population was of this ancestry does not indicate any decided drop in the future size of the group.

Probably the most interesting fact about this immigrant group is that, unlike the Americans, British, and Chinese, they have not intermarried freely with other peoples. Their outmarriages have been comparatively few. Adams explains this unusual restraint as being largely due to cultural considerations, a most important one being that Japanese marriages were arranged on the basis of equivalence of family status. This cultural attitude was preserved through the fact that the Japanese came in large numbers and thus were able to maintain their own social life with its customary sanctions. Adams remarks on their superior social morale and attributes to this their good record in the Territory. "Relative to numbers," he says, "there are fewer arrests and convictions, there are fewer juvenile delinquents, fewer who receive charitable aid, fewer insane, fewer who are mentally defective." The psychologist would, of course, suggest that at least some of these advantages are due not only to "superior social morale" but also to superior biological inheritance. Even their lessened tendency to crime and delinquency may also indicate superior temperamental qualities, and the incidence of mental defect certainly is not dependent on social morale. Granting the fact that the greater number of Japanese made social organization easier, we still are at a loss to find a sufficient explanation of the dearth of outmarriages, unless we also concede the view that these people are better disciplined through being more conscious of social pressure and having the will to obey.

In all other groups, especially that of the whites, Adams fully recognizes the strong urge to find mates outside their own group, if women of their own race are not available. "Public opinion," he states (p. 53), "never develops among men isolated from women of their own race." Again he remarks: "A man is so fundamentally a social being that it is much more important for him to enjoy something approaching normal human relationships than it is that he shall have his first choice as to the character of the people with whom he associates" (p. 123). To the

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reviewer, this seems like a polite sociological statement of a common saying in Northwest Australia: "Necessity is the mother of half-castes."

Though it is true, as Adams suggests, that brides of their own nationality were more easily obtained by the Japanese than by the Chinese, this advantage was, with regard to the great body of immigrants, more theoretical than real. By the end of the Nineteenth Century only 20% of the Japanese were married, while even by 1916 the Japanese sex ratio was more highly abnormal than the average for the general population. In other words, there was a fairly lengthy period during which Japanese women were not available as brides and yet in this period Japanese out-marriages were comparatively rare. Some other explanation than those usually advanced would seem to be needed to account for this extraordinary fact.

In the reviewer's opinion, social conformity is easy for some, difficult for others. Temperamentally, some people are submissive to discipline, willing to remain in step, and wary of adverse public opinion. All individuals are susceptible to cultural training, but some are more susceptible than others. The psychologist, I believe, is inclined to give more weight to the facts of individual and group differences than is the sociologist. Adams, however, does not neglect this factor and the reader will find in various chapters, but particularly in the chapter (XVIII) dealing with the character of the mixed bloods, considerable discussion of this topic.

For those who are interested in what is probably the most intensive, though circumscribed, racial contact and amalgamation that the world has known, the skillful observations of the sociologist on the spot will have extreme interest and value. Hawaii maintains on the rim of her most active volcano a laboratory under the charge of a renowned volcanologist. The sociologists at the University of Hawaii have a similar point of vantage on the very edge of this miniature melting pot.

Adams' observations are characterized by shrewdness, common sense, and scientific detachment. Except for some tendency towards repetition, the book is interesting reading and can be highly recommended to students of social change.

S. D. PORTEUS.

*University of Hawaii.*

SEABURY, D. *Adventures in self-discovery.* New York: McGraw-Hill, 1938. Pp. ix+324.

The professional psychologist will find nothing of importance here. Good precepts for living, much talk about the soul, the power of the unconscious, subliminal forces, cosmic energy, and something vaguely termed "neurosis"—all expressed in a style which is sometimes simple and clear but quite as often vague and mystical—do not make up a very impressive book. Where it is intelligible, it is commonplace; where it is not commonplace, it is obscure and verbose. The author has evidently read widely but uncritically. All forms of psychoanalysis (J. B. Rhine, Eddington, Millikan, and Bohr) appear to be accepted as equally reliable.

EDMUND S. CONKLIN.

*Indiana University.*

## BOOKS RECEIVED

AYAU, A. E. The social psychology of hunger and sex. Cambridge, Mass.: Sci-Art, 1939. Pp. 160.

GILBERT, M. S. Biography of the unborn. Baltimore: Williams & Wilkins, 1938. Pp. x+132.

JACOBSON, E. You can sleep well: the A B C's of restful sleep for the average person. New York: McGraw-Hill, 1938. Pp. xx+269.

KALLMANN, F. J., with the assistance of S. J. Rypins (with an introduction by N. D. C. Lewis). The genetics of schizophrenia: a study of heredity and reproduction in the families of 1,087 schizophrenics. New York: Augustin, 1938. Pp. xvi+291.

KÖHLER, W. The place of value in a world of facts. New York: Liveright, 1938. Pp. ix+418.

LATOUR, M. Premiers principes d'une théorie générale des émotions. (Nouvelle édition revue et augmentée: Observations complémentaires. Douzième série.) La stylisation au terme des formes et des concepts. Paris: Félix Alcan, 108, Boulevard Saint-Germain, VI\*, 1938. Pp. 60.

MÜLLER, G. E. Hegel über Offenbarung Kirche und Philosophie. München: Ernst Reinhardt, München 13, Isabellastrasse 11, 1939. Pp. 60.

RACE, H. V. The psychology of learning through experience. Boston: Ginn, 1938. Pp. viii+384.

RIESE, W., in collaboration with A. Requet. L'idée de l'homme dans la neurologie contemporaine. Paris: Félix Alcan, 108, Boulevard Saint-Germain, VI\*, 1938. Pp. vii+97.

SCHMIDT, F. Kleine Logik der Geisteswissenschaften. München: Ernst Reinhardt, München 13, Isabellastrasse 11, 1938. Pp. 128.

SKEELS, H. M., UPDEGRAFF, R., WELLMAN, B. L., & WILLIAMS, H. M. A study of environmental stimulation: an orphanage pre-school project. *Univ. Ia Stud. Child Welf.*, Vol. XV, No. 4. Iowa City: University, 1938. Pp. 191.

SMITH, B. O. Logical aspects of educational measurement. New York: Columbia Univ. Press, 1938. Pp. x+182.

VILLEY, G. La psychiatrie et les sciences de l'homme. Paris: Félix Alcan, 108, Boulevard Saint-Germain, VI<sup>e</sup>, 1938. Pp. 194.

WALTON, R. P. Marihuana: America's new drug problem. Philadelphia: Lippincott, 1938. Pp. ix+223.

WELLMAN, B. L. The intelligence of preschool children as measured by the Merrill-Palmer scale of performance tests. *Univ. Id Stud. Child Welf.*, Vol. XV, No. 3. Iowa City: University, 1938. Pp. 150.

## NOTES AND NEWS

DR. WILLIAM McDUGALL, since 1927 professor of psychology at Duke University and from 1920 to 1927 professor of psychology at Harvard University, previously reader in mental philosophy at the University of Oxford and a fellow of Corpus Christi College, died on November 28 at the age of sixty-seven years.—*Science*.

DR. HARRY R. DESILVA, lecturer in psychology at Harvard University, has been appointed research associate in psychology at Yale University. He will have charge of a program of Automobile Driver Research in the Institute of Human Relations. Research on drivers will be carried out by a staff in coöperation with neighboring motor vehicle departments. The work is made possible by a grant to Yale University from the recently established Esso Safety Foundation.—*Science*.

THE Sixteenth Annual Meeting of the American Orthopsychiatric Association, an organization for the study and treatment of behavior and its disorders, will be held at the Commodore Hotel, Lexington Avenue and 42nd Street, New York City, on February 23, 24, and 25, 1939. Communications relative to this meeting should be addressed to Dr. Norvelle C. LaMar, Secretary, 149 East 73rd Street, New York City.

THE Washington-Baltimore branch of the American Psychological Association held its first meeting of the year at the Catholic University of America in Washington, D. C., on November 9, in connection with the celebration of the fiftieth anniversary of the founding of that University. The following program was presented:

- C. J. CONNALLY, Catholic University: "Physique and Psychoses."
- J. P. FOLEY, JR., George Washington University: "Effect of Prolonged Inverted Retinal Stimulation upon Spatially Coördinated Behavior in the Rhesus Monkey."
- T. V. MOORE, Catholic University: "Theory of Perception in the Light of Pathology and Experimental Psychology."

A CONFERENCE on the Educational Production of Motion Pictures, sponsored jointly by the College of Education, Ohio State University, the National Council of Teachers of English, and the Film Project of the American Council on Education, was held on the campus of the Ohio State University on November 22 and 23, 1938. The program included demonstrations, discussions of techniques, and talks on the application of films to the various fields of education.



THE Institute of Educational Research, Teachers College, Columbia University, has completed the Semantic Word Count. This study, which was carried out under the direction of Dr. Edward L. Thorndike, lists all the words of more than 1 meaning which occurred in a sample of 2,500,000 words. It gives an estimate of the number of times a particular meaning is likely to occur in 1000 occurrences of a word. It also notes the number of different types of material in which that meaning is to be found; that is, its range of occurrence.

The Semantic Word Count furthers the principle of economical learning which was inherent in the publication of Dr. Thorndike's first count, *The teachers word book*. That book lists the 20,000 most common words and has been used as a guide for those who write or edit school readers and other textbooks. Educational psychology has by now accepted the idea that the more common words are the most important for a child to know.

There would be no further problem if all English words had but 1 meaning. But since this is not so, it becomes equally important to know which meanings or concepts of a word are the most important to teach. The Semantic Count offers the first quantitative approach to this problem.

Aside from its importance in grading readers, the Count provides an opportunity for the editors of school dictionaries to improve their product. There has always been a twofold problem for school lexicographers: which meanings to include and in what order meanings should be placed. The Semantic Count offers the solution to both of these questions.

The Semantic Count will also provide a useful instrument of research in the fields of language, psychometry, and adult education, especially the teaching of English to adult foreigners. Much interest has been expressed in the project by educators in charge of the program of teaching English as a secondary language in such places as India, Egypt, and the United States Territories.

THE Inter-Society Color Council will hold its annual meeting February 23, 1939, in the auditorium of the Electric and Gas Association of New York, 480 Lexington Avenue, New York City. The morning session will deal with the business of the Council, which will include the reports of committees through which the activities of the Council are carried on. In the afternoon and evening the following programs of invited papers of general color interest will be offered:

2:00 P.M. TECHNICAL SESSION ON COLOR TOLERANCES

This meeting is sponsored jointly by the Inter-Society Color Council and the American Psychological Association.

FORREST LEE DIMMICK, Hobart College, Chairman

*The Physics of Color Tolerances.* DEANE B. JUDD, National Bureau of Standards. The physicist evaluates color differences operationally in a standard coördinate system; perceptibility of color difference is relatively unimportant in tolerance consideration.

*The Psychophysics of Tolerances.* EDWIN G. BORING, Harvard University. The fundamental significance of the psychophysical methods and techniques in the evaluation of small sensory differences.

*The Ratio Method in the Review of the Munsell Colors.* SIDNEY M. NEWHALL, Johns Hopkins University. A promising application in a different field of a psychological method which is being used in color tolerance determination.

*Color Tolerances as Affected by Changes in Composition and Intensity of Illumination and Reflectance of Background.* HARRY HELSON, Bryn Mawr College. Typical data illustrating lawful relationships between the hue, saturation, and lightness of surface colors and principal conditions of viewing.

*Representation of Color Tolerances on the Chromaticity Diagram.* DAVID L. MACADAM, Eastman Kodak Company. The ICI coordinate system is recommended for color tolerance representations; use of a coordinate system based on just noticeable color-difference data would be unjustified and misleading.

*Specification of Color Tolerances at the National Bureau of Standards.* DEANE B. JUDD, National Bureau of Standards. Color tolerances have been applied by (1) specification of permissible area on a mixture diagram, (2) use of a standard and tolerance sample, and (3) use of the 'NBS unit of surface-color difference.'

*Industrial Color Tolerances.* ISAY BALINKIN, Cambridge Tile Manufacturing Company, Cincinnati, Ohio. Techniques and results in the establishment of color tolerances for particular products and purposes, and with special reference to ceramics. Control of color uniformity by determination of rates of color variation as a function of various physical or chemical factors.

#### COLOR TOLERANCE EXHIBIT

There will be an exhibit of color tolerance problems in connection with the afternoon program.

8:00 P.M.

#### POPULAR SESSION: PARADE OF COLOR

M. REA PAUL, National Lead Company, Chairman

Each member body has been invited to contribute a short demonstration of recent color developments or of a research project of outstanding importance to its field. The purpose is to illustrate the color interests of each one of the member groups.

Demonstrations are assured of color in medicine; color in psychology; color in textiles; color in paper; color in lighting; and color in fashion.

Every effort will be made to present each story as dramatically as possible. It is appreciated that the evening session will attract many whose color experience encompasses only one limited sphere, and that their primary interest will be to understand how color is used in other fields. By this method a mutual appreciation of the future possibilities in coordinated color work will be engendered.

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